PUBLIC LIBRARY

SCIENTIFIC AMERICAN

No. SUPPLEMENT 437

Scientific American Supplement, Vol. XVII., No. 437. | NEW YORK, MAY 17, 1884. | Scientific American Supplement, $5 a year.
Scientific American, established 1845. | | Scientific American and Supplement, $7 a year.

BANGE'S ARTILLERY SYSTEM.*

II.—SIEGE AND FORTRESS GUNS.

DURING the course of a war, an army that is obliged to lay siege to a fortress has need for such purpose of more artillery *materiel* than it carries along with it in the field. The time that it takes to get such *materiel* together, and the expense and trouble that result therefrom, cause the attacking of fortresses to be avoided as far as possible.

Operations of this kind, which are always very difficult, and the consequences of which are often grave, are only undertaken when their absolute urgency and necessity are recognized.

The fighting and reducing to silence of the powerful artillery of the besieged place, comprising as a general thing pieces of the largest caliber, the rendering of the *terre-pleins* of the fortification untenable and the destroying of the places of shelter established thereon, the ruining of all the defenses of the place, and the opening from a distance (if possible to do so) the ramparts to the besieging army by destroying the escarps and overturning the ramparts into the ditch, so as to form slopes practicable to the assaulting columns—such is theoretically the role of siege artillery, which, for that reason, must be possessed of great power.

The condition of *mobility*, so necessary in field artillery, is here of less consequence. The long operations connected with a siege require the army to have a railway at its disposal in its rear, and it follows from this that the difficulty of transporting large calibers is partially overcome. It will suffice if the *materiel* can be carried without much trouble from the railway station where it is unloaded to the works established by the attacking party. It would be right, then, to exclude from the siege *materiel* only pieces of a really exceptional weight.

In the month of August, 1874, the artillery committee submitted to the Minister of War, for his approval, a project for the creation of a new siege *materiel* comprising rifled cannons of 120, 155, and 220 mm. caliber, and rifled mortars of calibers of 220 and 270 mm. All the guns were to be made of French steel and to be breech loaders. After some conclusive experiments, the minister adopted, in 1877, the cannon of 155 mm. (Bange system), in 1880 the cannon of 120, and in 1881 the rifled mortar of 220 mm. of the same system.

The 155 mm. (6 inch) Bange gun is of steel, rifled to the right (48 progressive grooves), and loads at the breech. This piece, weighing 2,500 kilogrammes (5,500 lb.), throws, with a charge of 8·75 kilos (19¼ lb.) of powder, a projectile weighing 40 kilos (88 lb.). The initial velocity that corresponds to such charge is 464 meters (1,522 feet), and the maximum range of the projectile, at an angle of 28° is exactly 9·1 kilometers (5·6 miles).

The 155 mm. (6 inch) cannon consists of two principal parts, viz., the body and the breech mechanism. The former consists of a cast steel tube, hammered, bored, and tempered in oil, and of two thicknesses of hoops for strengthening the back part. These hoops, which are of puddled steel, are made by rolling, and are tempered. Those of the first row, ten in number, are placed upon the tube, which has been turned to a diameter of 0·30 mm. This first row is covered at its hind part, which is turned to a diameter of 0·37 m., with a second row of six hoops. Upon that part of the re-enforce which is formed by the fourth hoop from the edge of the breech there is adjusted a seventeenth hoop called the leveling hoop, which is likewise of steel, and which carries a ring underneath. The breech mechanism and the obturator are, with the exception of a few details, a reproduction on a large scale of the same parts in the 90 mm. field gun.

At present the 155 mm. cannon throws two kinds of projectiles—the ordinary and the ball shell. The former of these is a cylindro-ogivoid projectile 0·465 m. (about 18¼ inches) in length. Charged, and primed with a siege percussion fuse (model of 1878), it weighs, as before stated, 40 kilos. The weight of the interior charge is 1·7 kilos of common powder.

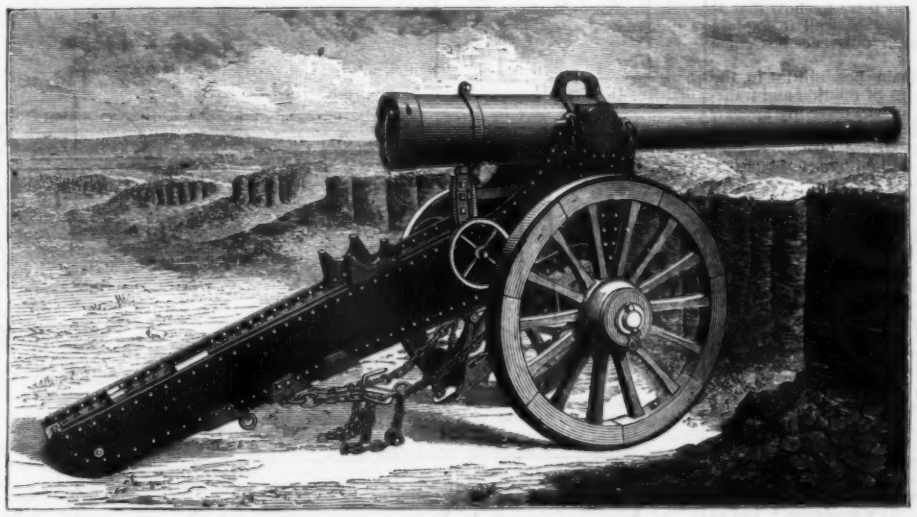


FIG. 1.—BANGE SIEGE GUN OF 155 MM. CALIBER.

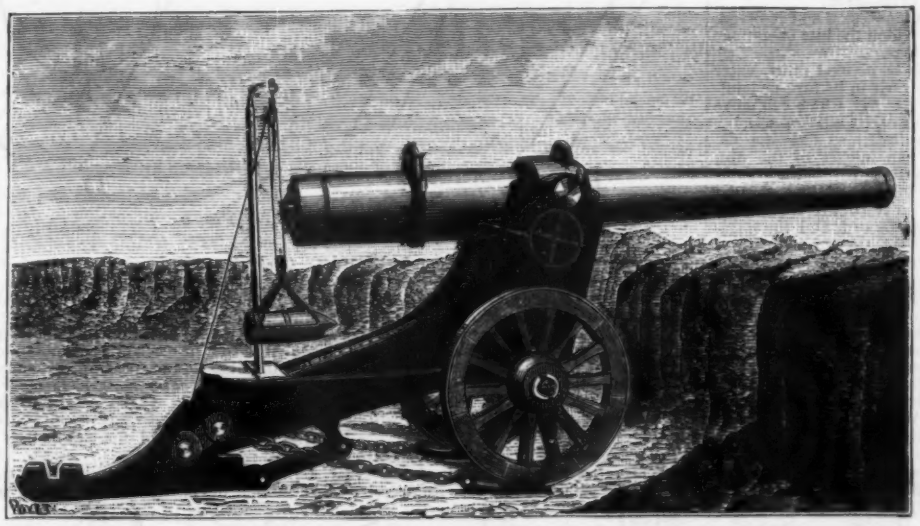


FIG. 2.—BANGE SIEGE GUN OF 220 MM. CALIBER.

FIG. 3.—BANGE RIFLED MORTAR OF 270 MM. CALIBER.

The ball shell is a cast iron projectile of the same form as the other, and differs from it externally only in being shorter, and internally in its larger capacity. Its charge consists of 450 grammes of powder and 270 balls 16·7 mm. in diameter and weighing 26 grammes. These balls, after being introduced, are surrounded with melted sulphur. Thus charged, and primed with a double-acting siege fuse, the ball shell weighs about 41 kilos.

The maximum charge, which, as above stated, is 8·75 kilos, is inclosed in an amiantine cloth bag. Besides this, smaller charges of 7, 6, and 5 kilogrammes are employed.

The 120 mm. (4·6 inch) cannon is a steel piece rifled to the right (36 progressive grooves) that loads at the breech. It weighs 1,200 kilos (2,640 lb.), and, with a normal charge of 4·5 kilos (10 lb.) of powder, throws a projectile weighing 17·8 kilos (39 lb.). The initial velocity corresponding to this charge is 480 meters (1,574 feet), and the maximum range of the projectile is 9·2 kilometers (5·7 miles).

The body of this cannon is formed of a steel tube, hammered, bored, and tempered in oil, and of seventeen hoops that re-enforce its entire length and gradually diminish in thickness toward the muzzle. As in the preceding gun, these hoops are of puddled, tempered steel. The breech mechanism and the obturator are the same as before. The projectiles thus far adopted are the ordinary shell and the canister. Eventually, it is likely that a ball shell will be put in service.

The ordinary shell is of the same shape as the preceding, and is 0·35 m. in length. The cylindrical part is united to the ogive by a bulge, which, in the rough-cast shell, is 0·121 m. in diameter. Eventually, a portion of this projection is removed in the lathe and converted into a cylindrical surface of a diameter of between 118·9 mm. and 119·3 mm. It is through this turned part, 32 mm. in length, that the front of the projectile rests against the unrifled parts of the gun. All charged, and primed with its fuse, the ordinary shell weighs 17·8 kilos. The weight of its charge is 800 grammes of common powder.

The canister shot is a cylinder of laminated zinc, 2·5 mm. in thickness, closed at one extremity by a cast zinc bottom 25 mm. in thickness, and at the other by a cover of cast zinc only 15 mm. in thickness. Upon the bottom there is placed a wooden disk covered with a disk of iron plate provided with lugs. This box contains 282 hard lead balls (composed of nine-tenths lead and one-tenth antimony) 20 mm. in diameter and weighing 44 grammes. The normal charge of powder (4·5 kilos) is inclosed in an amiantine cloth bag. Besides this, a smaller charge of 4 kilogrammes is employed.

The 220 mm. (8·6 inch) mortar of the Bange type is a steel piece rifled to the right (60 progressive grooves) that loads at the breech. It weighs 2,000 kilos (4,400 lb.) and, with a maximum charge of 6 kilos (13 lb.) of powder, throws a projectile weighing 100 kilos (220 lb.), including an internal charge of 8 kilos (17¼ lb.) of powder. The initial velocity is then 250 meters (820 feet), and the maximum range (corresponding to an angle of 30°) is 7 kilometers (4·34 miles). Being specially designed for firing at large angles, this gun is mounted upon a special carriage, which is wholly of metal, and which has no analogy with the carriages of the 155 and 120 mm. guns.

In field artillery the vehicle formed by uniting the gun carriage with a fore-carriage is so arranged as to carry all the accessories necessary for use about the gun. In siege artillery the gun is more powerful and consequently heavier. It must be mounted on a battery at a site

* Continued from SUPPLEMENT, No. 436, p. 6791.

that has been selected and arranged in advance, so the accessories that are necessary for it are independent of its carriage, and the latter is provided with neither a fore-carriage nor an ammunition chest. Such accessories, then, as tools and fittings occupy a place apart in siege batteries, and the same is the case with the ammunition and projectiles, which are prepared under cover in the vicinity of the guns. Some of these tools and equipments may remain exposed to the open air. Such are the leveling instruments, the maneuvering levers, the swabs, the muzzle cover, sight cover, etc. Other apparatus require to be kept under cover at all times, and for this reason are stored away in cases arranged for the purpose.—*La Nature.*

PLANETARY WHEEL-TRAINS.

By Prof. C. W. MACCORD, Sc.D.

I.

THE action of those curious combinations of wheels, known as epicyclic or planetary trains, has been discussed at considerable length by various writers; yet there seem to be unnecessary, not to say confusing, complications in the investigations, in the respect that the same formulæ do not apply in all cases to trains composed of different kinds of wheels: and again, certain distinguishing features of different kinds of trains have not been, we think, as clearly defined or strongly emphasized as they should be.

Now, if a certain formula or equation expresses the action of a given planetary train of spur wheels, there certainly does not appear to be, upon the face of it, any reason why the same equation should not apply equally well to a similar train made up of bevel or of screw wheels. And with a proper understanding of terms, we shall try to show that it will: but as a preliminary step it may be well to explain how it is that, in some cases at least, a difference has come about.

This difference grows out of certain possibilities in the composition and resolution of motions of rotation and translation, which have also a bearing, as will presently appear, on other mechanical questions than the one immediately under discussion: and in order to avoid any misunderstanding, we shall begin at the very foundation, by defining a few terms which must be frequently employed.

1. A point is said to *revolve* about a right line as an axis, when it describes a circle of which the plane is perpendicular to, and the center lies in, that line.

2. Any geometrical magnitude is said to revolve about a right line as an axis, when all its points revolve about that line as above defined with the same angular velocity, and therefore without change of their relative position.

3. Any geometrical magnitude is said to have a motion of *translation* when all its points describe similar and equal paths: if these paths be circles, the motion is called one of *circular* translation.

4. *Distinction between* Revolution *and* Rotation.

If the axis pass through the center of a revolving body, as in the case of a circular wheel, the term *rotation* may be used as synonymous with revolution. But a wheel thus turning about its own axis of symmetry may also travel in an orbit about another axis in the same or a different plane: in which case, to avoid confusion, we shall designate the former motion a *rotation*, and the latter a *revolution*; just as we say that the earth rotates upon its axis while it revolves around the sun.

5. *Composition* of Rotation *and* Translation.

These circular motions may be compounded and resolved, in a manner very similar to that in which the like operations are performed with rectilinear motions.

Thus in Fig. 1, let the disk LVM be pivoted at its center B to the rigid bar IAB; and let this bar in its turn be jointed at I and at A to the two cranks IH, AC, which can move only by revolving about the fixed axes HC. Then the bar IB has a motion of circular translation, like the parallel rod of a locomotive, and can impart to the disk no other motion. Let the cranks turn through an angle ACK: then the center B will have gone to E, in a circular path whose center is D, situated with respect to B just as C is with respect to A, or H with respect to I, and the angle BDE will be equal to ACK. Similarly, the points L, X, M will have gone to N, Y, and O, respectively, the path of each being a circular arc equal to BE, and of the same radius: and the arrow LM will retain its vertical position, or in other words maintain its parallelism to itself and to its original direction, throughout the action.

Next let us suppose that the disk is first turned about its center B, through an angle LBV equal to ACK, and afterward moved by translation as before. The arrow will in this case remain parallel to the direction UV, and when B reaches E, will have the position ZW. The result is the same as though the arrow had, as originally supposed, been moved by translation from the position LBM to the position NEO, and subsequently rotated about E through the angle NEW, equal to LBV, ACK, and BDE.

FIG. 4.

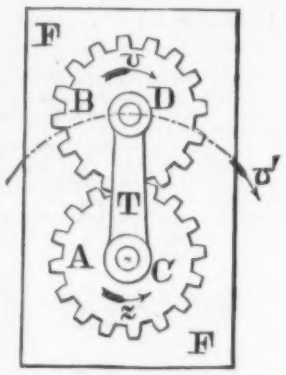
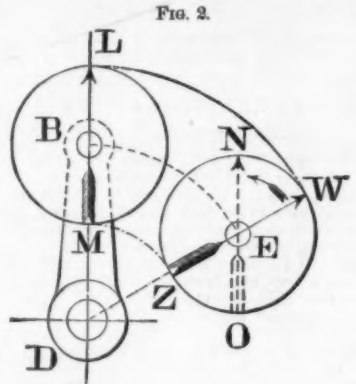


FIG. 2.



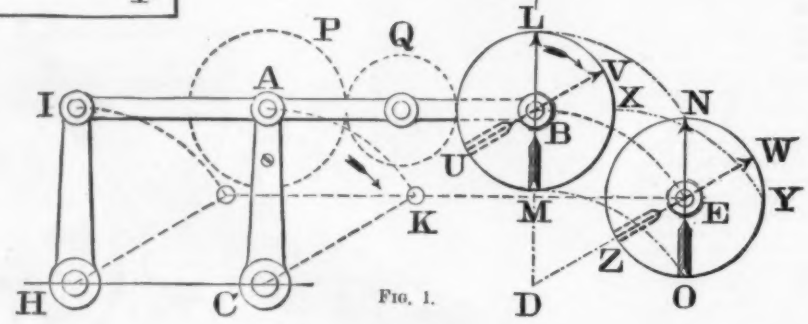


FIG. 1.

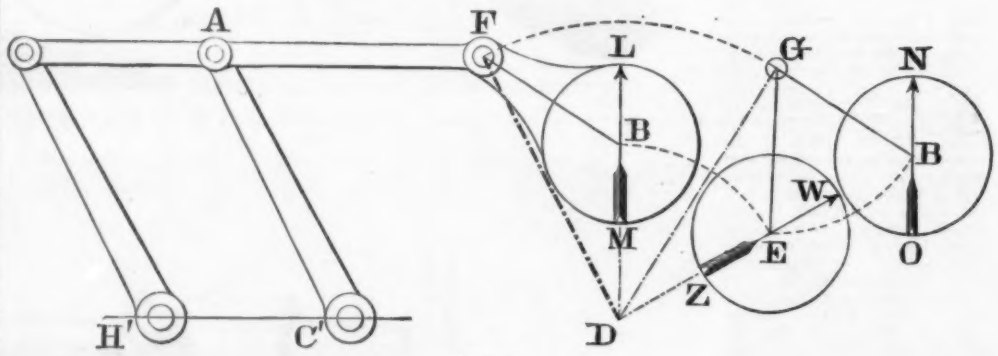


FIG. 3.

BE as before, and that of M the arc MZ; all which arcs have the same center D, and subtend the same angle BDE.

In other words, the resultant of the assigned motions of rotation and circular translation is a motion of *revolution* about the fixed axis D.

Conversely, the revolution about the fixed axis D, represented in Fig. 2, *may* be resolved into the components of rotation and translation represented in Fig. 1; but it may also be resolved into other components, as illustrated in Fig. 3. Here the center D is situated in reference to B exactly as in the preceding diagrams, but the disk by means of a rigid arm, or extension, is pivoted to a bar FA'I', not at its center B, but at a different point F. The fixed axes C' H' are situated in reference to A' and I' respectively just as D is in reference to F. Suppose the cranks I'H', A'C', to revolve in a horizontal plane, and the pivot at F to be frictionless; then, as in Fig. 1, the action of this arrangement will be to give the disk a motion of circular translation, the point F going to G, and the arrow LBM to the position NB'O, remaining always vertical. Let the angle FDG be equal to the angle BDE of Fig. 2; and after this translation let the disk rotate about the the axis G in the same direction, through an angle B'GE equal to FDG. The result is, that the final position of the arrow, WEZ, will be the same as in Fig. 2.

The position of F, it is apparent, is entirely arbitrary; and, observing that all the axes, fixed or moving, are perpendicular to the planes of rotation, we perceive that a motion of revolution about a fixed axis may be resolved into two components, one of which is a rotation about *any axis parallel to the fixed one*, and the other a circular translation in the same direction and of the same angular velocity, in which the radii of the circular paths are equal to the distance between the axes. It should also be noted that because the traveling axis about which the rotation takes place *is* parallel to the fixed one, its motion about the latter is the same, whether it be called revolution or translation, these two being in this case identical. And again, it is to be observed, that this resolution and composition is possible *only* when the fixed and the moving axis are absolutely parallel. Of course, a geometrical magnitude may rotate about an axis which has any motion whatever in relation to another fixed axis; but the resultant will not be a *revolution* about the latter, in any other case than the one above discussed.

Finally, the motion of circular translation, also, may be resolved into components. If in Fig. 2 the disk be pivoted to the crank at its center B, then the only motion which the crank can give the disk is one of revolution about D, the arrow going from position LM to position WZ. But if meantime the disk be caused to rotate about the pivot B, with the same angular velocity but in the opposite direction, the resultant will be a motion of circular translation, the arrow remaining constantly vertical and taking the final position NO. Neither the revolution nor the translation, then, can be regarded as a simple motion essentially—either one may, like a rectilinear motion, be resolved into other component motions, and be regarded as their resultant.

These principles being understood, we may now proceed to consider their application in the analysis and explanation of mechanical combinations, and more particularly in respect to the action of planetary trains.

The most elementary form of such a train is shown in Fig. 4; the crank, or *train-arm*, in which the wheel B has its bearings, turning above the fixed axis C of the wheel A: and for simplicity's sake let these wheels be of equal size.

Before considering the planetary action let us for the moment suppose the train-arm to be stationary, and the

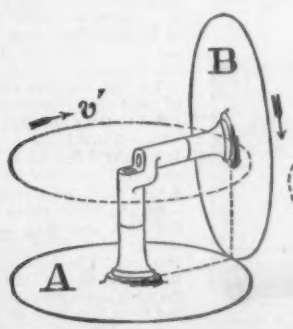


FIG. 5.

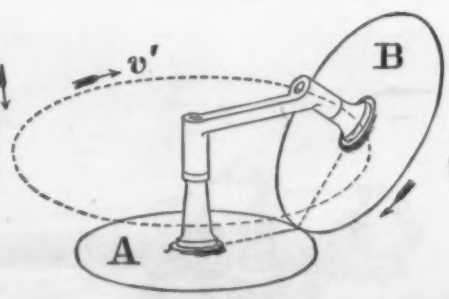


FIG. 6.

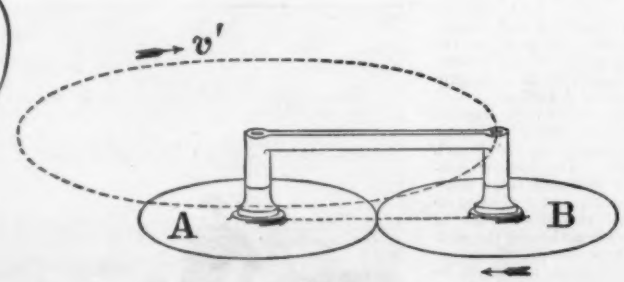


FIG. 7.

Now, these two motions, instead of taking place successively, may take place simultaneously; we may imagine the disk to turn about B, while the crank AC turns about C, in the same direction and with the same angular velocity. The arrow will no longer remain parallel to itself; but being drawn parallel to AC originally, it will always be so, and when B reaches E, will as before have the position ZW.

By comparing this diagram with Fig. 2, it will be at once seen that the same result would follow, were the disk rigidly fixed to the crank BD shown in the latter figure, which is capable of moving only by revolving about the fixed axis D: the actual path of L being the arc LW, that of B the arc wheel A to make one rotation about C to the left, as shown by the arrow Z. It is then evident that B will in the same time make one rotation about D to the right, as shown by the arrow V. Each wheel, then, has made one rotation about its own axis, has turned once in its own bearings, and once relatively to the crank.

Next, let the wheel A be fixed to the frame F, while the crank makes one revolution about C to the right, as shown by the arrow V'. How many rotations about its own axis does the wheel B make during this orbital motion? Simple and even trivial as this question may appear, it has at various times provoked almost acrimonious discussion; and

at any rate, it is upon the answer to it that the further prosecution of our investigation depends. And again, since the answer which we deem the proper one is at variance with that sanctioned by authorities not lightly to be disputed, we deem it obligatory to quote their views thereupon.

First, however, it may be observed that there is no question or difference of opinion in respect to certain physical facts. It is evident that since either of two bodies may be considered at rest with respect to the other, the revolution of the crank about C is equivalent, so far as the wheel A is concerned, to the original rotation of that wheel in the opposite direction: and that since the two wheels engage tooth for tooth as before, the wheel B turns once in its bearings, or relatively to the train-arm.

But Prof. Goodeve says (*Elements of Mechanism*, p. 158)

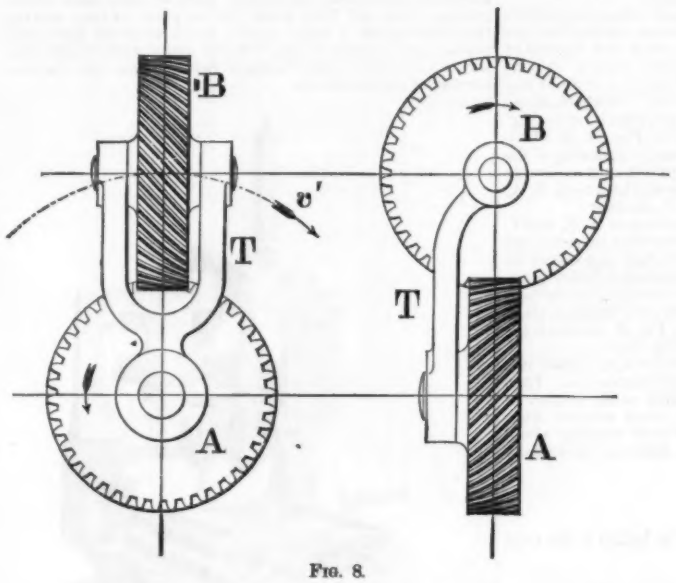


FIG. 8.

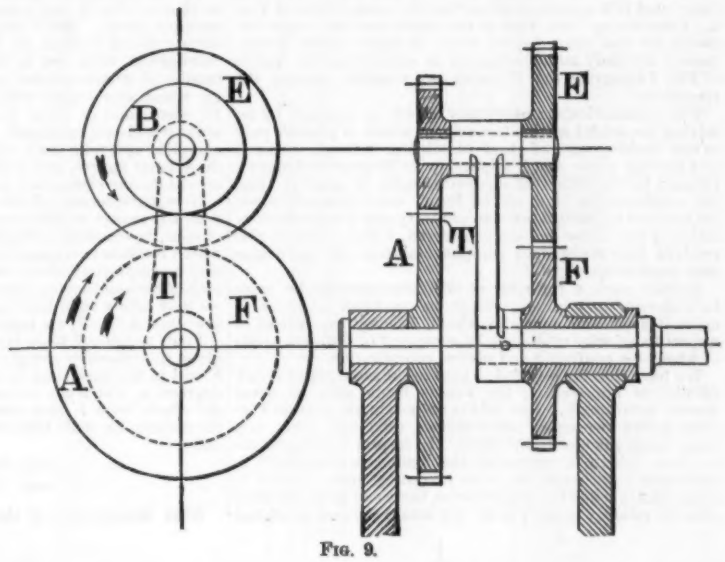


FIG. 9.

"this is only a part of its movement; the wheel B has also been carried round in a circle about A by reason of its connection with the arm, and *has turned upon its axis once more on that account.*" (The italics are ours.) Then, supposing A to be removed altogether and B to be locked to the arm, he says further:

"As the arm goes round, a person inspecting the apparatus from a little distance will see the wheel B turning on its axis, and if he watches a mark upon the rim, he can entertain no doubt about this fact. The truth is that, although the wheel B does not move relatively to the arm, it is nevertheless the subject of two distinct motions, whereof one consists in a revolution about an axis passing through its center, and the other is a motion of *translation*, as it is called, whereby the center of the wheel describes a circle whose radius is the distance between the centers of A and B. If, again, the wheel B were looked at from the center about which the arm revolves, no motion of rotation could be recognized. The very same thing happens in the case of the moon. Astronomers tell us that . . . the moon turns once upon its axis during the period of a single revolution in its orbit round the earth: or in other words, that it moves as if it were fixed to a solid bar stretching from the earth to the moon."

According to Prof. Goodeve, then, the planet-wheel B makes *two* rotations about its own axis during the revolution of the train-arm: and he subsequently constructs a formula for the action of planetary trains upon that basis.

translation is essentially a simple one. We have already seen that it is not, but may itself be regarded as the resultant of a revolution and a rotation. And though no motion can be called *simple*, in the sense that it cannot be resolved, it may be remarked that if any motion in mechanism is worthy of being called simple, by reason of the ease with which it can be produced and continuously maintained, the motion of revolution about a fixed axis is entitled to that distinction.

2. The revolution mentioned can be resolved into a rotation about *any* axis parallel to the fixed one, combined with a corresponding circular translation. To insist, then, upon the particular resolution designated above is to make with no apparent reason a preferred creditor of the axis passing through the center of the crank-pin, leaving unsatisfied other equally just and pressing claims to the number of infinity minus one.

3. The shaft may be enlarged until it swallows up and surrounds the crank-pin. This leads to the conclusion that any circle, as for instance a wafer, on the end of the shaft, must be considered "the subject of two distinct motions" as expressed by Prof. Goodeve; which seems inconsistent with the statement that "the motion of the shaft consists simply in a rotation about its own axis."

4. The astronomical illustration of Prof. Goodeve is not a correct one, inasmuch as it disregards the moon's libration in latitude. The explanation of the astronomers, that the moon makes one rotation on its axis while revolving once

has not received the pragmatic sanction of the engine itself. Did the crank pin receive a motion of translation from one source, and one of rotation about its axis of symmetry from another, the resultant would be the same as the motion which it actually has. But this is conspicuously not the case; and we protest against the statement that the crank pin "is the subject of two distinct motions," as being contrary to the existing *status rerum*, and as tending to convey an incorrect idea of what really occurs.

To us it seems perfectly clear that in describing the operation of any mechanical device, we are bound to state what *is*, and not what might be; just as much as we are in describing its construction. If this be so, it is not proper to say that any given piece is the subject of two distinct motions, unless the mechanism be so constructed *that those two motions are imparted to it by independent means.* To illustrate: if we imagine that in Fig. 1, the disk B is driven by a train of clockwork carried by the bar to which the disk is pivoted in the same direction and at the same rate as the two cranks; then if we stop the clock, the disk has a motion of translation only, if we stop the cranks it will have the motion of rotation only, but if both clock and cranks operate simultaneously, the resultant is a motion of revolution about D. Under these circumstances, it appears to us that the action of the arrangement *can* be correctly described by stating that the disk receives two distinct motions, and not in any other way.

In the above illustration, the clockwork was supposed to be driven by a spring, and to be in no way connected with the cranks. But we do not conceive such absolute independence to be essential. Suppose B to be provided with teeth, in short to be a spur wheel; let another of the same size, represented by the dotted circle P, be rigidly fixed to the crank AC, concentric with the crank pin A; and let Q be an idle wheel engaging with both and turning freely on a stud fixed in the bar IAB.

The rotation of the disk is now so far independent of the circular translation, that if P or Q be removed it will cease, and the latter motion alone will remain; and we should still consider these two motions as distinct, inasmuch as one of them requires the presence of parts whose removal does not destroy nor in any way affect the other.

Now, it is very certain that neither of these contrivances is

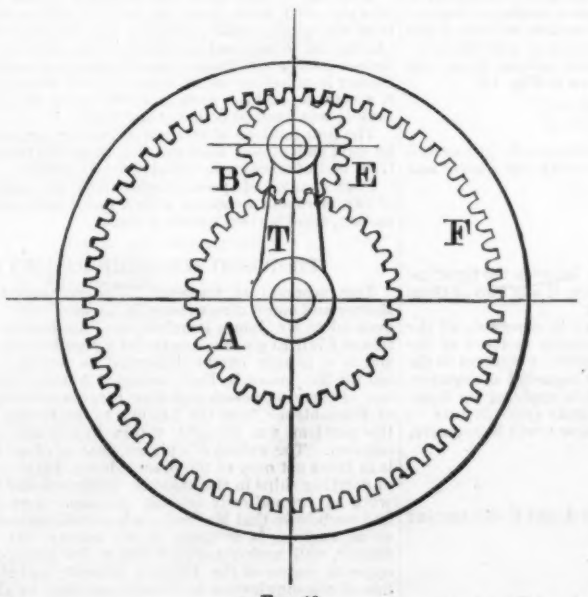


FIG. 10.

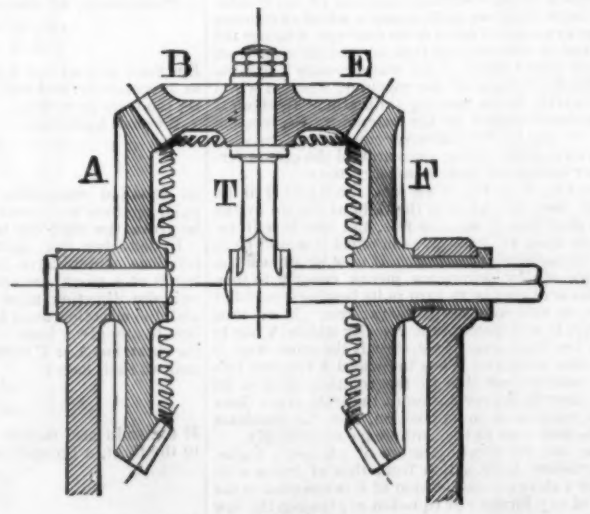


FIG. 11.

Now, the supposition last made (that the wheel A is removed and B locked to the arm) reduces the system to the form shown in Fig. 2, if we suppose the disk B to be fastened to the crank; and this is exactly the state of the crank-pin of a steam engine: in reference to which we quote from Prof. Rankine (*Machinery and Mill Work*, p. 37):

"Consider, for example, that rotating piece in a steam engine which consists of the shaft, crank, and crank-pin, and which turns about the axis of the shaft, as a fixed axis of rotation, to which the axis of the crank-pin is parallel. Then the motion of the shaft consists simply in a rotation about its own axis; while the motion of the crank-pin con-

in its orbit, is correct, of course; and it is the only admissible one, since the moon's axis has a true motion of translation, remaining constantly parallel to itself: but that axis is not perpendicular to the plane of its orbit, and in consequence this translation is not a revolution about any other line, and our satellite does *not* move "as if it were fixed to a solid bar stretching from the earth to the moon."

5. Once more referring to the crank pin, we would remark that as a physical fact it receives but a single motion, which is determined and controlled entirely by the bearings of the main shaft; and that the resolution of this motion into its components, an imaginary and purely arbitrary proceeding,

identical with a simple disk fastened to a single crank, as shown in Fig. 2; the resultant motion of the disk is the same in all three, but in the latter arrangement, neither of the component motions can be produced independently of the other.

The above will suffice to show, we think, that the considerations relating to the resolution of motion, mentioned at the outset, have a bearing upon the question of what constitutes accuracy in describing the action of mechanical combinations; we shall now try to prove that they also have a bearing upon the nature of mechanical equivalence in different combinations, at least of certain kinds.

Take for example the three pairs of wheels shown in Figs. 5, 6, and 7, and for simplicity's sake let all the diameters be equal. First, let us imagine the axes to be fixed; then in respect to velocity ratio, these are manifestly equivalent combinations—in any one of them, a rotation of the wheel A about its axis will produce one rotation of the wheel B about *its* axis.

Now, we confess our inability to see any reason why these trains are any the less equivalent, when transformed into planetary ones, by supposing A to be fixed and B to revolve around the vertical axis. The bald absurdity of asserting that the planet-wheel in Fig. 7, while circumnavigating the fixed central wheel, in any physical or mechanical sense, actually does or accomplishes anything more than it does in Fig. 5 or Fig. 6, is too apparent to require discussion. Now, there is no question or dispute as to the fact that in Fig. 5 the wheel B makes *one* rotation about its axis while revolving once; and it is equally obvious that the same is true of Fig. 6. Considering then that in the latter case the angle between the two axes may be made as small as we please, there is certainly an inconsistency in explaining the action of Fig. 7 by saying that B makes *two* rotations during the revolution.

This explanation, as previously shown, is reached by resolving the orbital motion in a manner which is possible only in this limiting case of exact parallelism between the fixed and moving axes; and it appears to us illogical to deduce a formula for the action of epicyclic trains in general, from the consideration of a special form; more especially since the analysis is founded upon an arbitrary and imaginary resolution of one of the motions considered, which might be also resolved into many other components than the particular ones insisted upon.

Besides, such a formula, as will subsequently be more fully shown, must in some cases be modified in order to make it applicable to trains which, although equivalent in respect to velocity ratio, may be composed of different kinds of wheels—a confusing and useless complication.

We have thus far referred to axes in the same plane, either parallel or intersecting, the wheels being spur or bevel wheels accordingly. But this is not essential; thus in Fig. 8 are shown two equal screw wheels, each axis lying in a plane perpendicular to the other. In respect to velocity ratio, then, this train, supposing the train arm to be fixed, is equivalent to either of the three preceding ones. And if A be stationary while the arm revolves, there can be no question as to the rotatory velocity of B· nor would the case be altered

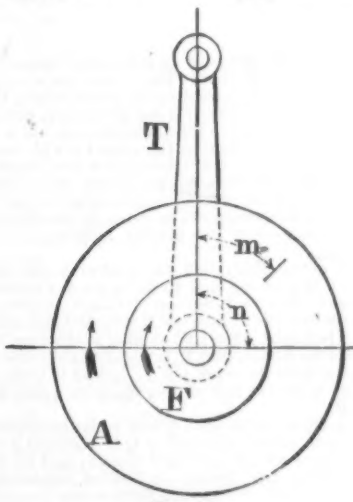


FIG. 12.

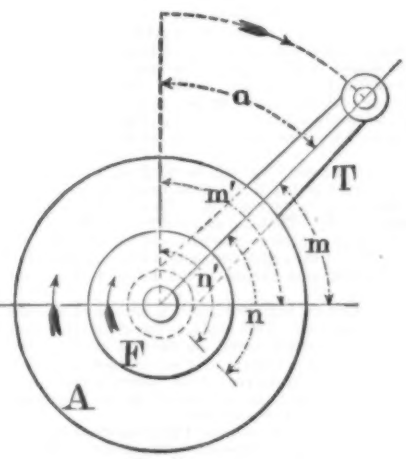


FIG. 13.

by changing the relative angular position of the axes, nor by substituting skew wheels for the pair here shown; the planet wheel in either case rotates once, neither more nor less, about its own axis during each revolution about the fixed sun wheel.

For the purposes of our own investigations of the combinations in question, then, we shall regard a wheel as rotating about its own axis *when it turns in its bearings*, whether the bearings be fixed or movable; so that, as will be clear from the illustrations above given, a sun wheel rotates if it turns relatively to the fixed frame of the machine, a planet wheel if it turns relatively to the moving train arm; all which is substantially comprehended in the heterodox asseveration that a wheel of any kind whatsoever makes *one* rotation about its own axis while rolling once around the circumference of another having the same number of teeth.

If in either Fig. 4 or Fig. 8 we lock the wheel B to the train arm, and turn the latter to the right as shown by the arrow e^1, it is clear that A must be free, and that it will rotate as though fixed to the arm, as in effect it would be in that case by the engagement with B. If now we unlock the latter wheel, we can by extraneous means cause it to turn relatively to the arm, that is, to turn in its bearings, in either direction, just as well as when T was at rest. If we thus turn B one way, it will increase the velocity which A has in common with the train arm; if we turn it the other way, it will diminish that velocity. Thus the wheel A receives two independent motions, one due to the rotation of B in its bearings, the other to the revolution of the train arm; these may be in conjunction or in opposition, and the resultant motion of A is their sum or their difference accordingly.

This is the one prominent feature of planetary trains, which distinguishes their action from that of trains with fixed axes, and a clear comprehension of it is essential to the investigation of any formula or equation expressing the law of their action.

Now, the train arm being extended, may carry a train of any number of wheels, but the vital principle of the most complex arrangement is embodied in the single sun wheel with its solitary planet, to which our attention has been confined. If, however, only one central wheel be employed, the functions of the mechanism will have a very limited range. The outer wheels might be driven by means of a spring connected with the train arm, but such a device is clearly not practical, and if on the other hand the central wheel and the train arm be independently driven, the effect is confined to controlling the velocity of the axial motions of the wheels carried by the arm, and the chief application of the arrangement would be, probably, in illustrating the motions of the solar system.

But there is nothing to prevent the introduction of another sun wheel, having the same axis as the first, as shown in Fig. 9, which represents what may be called a *complete* epicyclic train. Here a second planet wheel E is fixed upon the same shaft with B, and rotates with it; this engages with the second sun wheel F, which has the same axis as A, but moves independently of it; as shown in the sectional side view, this wheel F has a tubular shaft, supported in a bearing in the frame of the machine, within which the shaft of the train arm T revolves.

In this case, it is clear that we may drive either sun wheel and also the train arm, in either direction and at any speed, by independent means; and that the motion of the other sun wheel will be thereby determined. Or, we may assign both the direction and the velocity of the two sun wheels, which will determine the motion of the train arm.

This arrangement consists of two such elementary trains as that of Fig. 2, the connection between which is in this example direct. But it will readily be seen that instead of having B and E fixed on the same shaft, motion may be transmitted from one to the other through a train of any number of wheels carried by the train arm. Or again, a single wheel may engage with both the sun wheels, as in Fig. 10, where one of these is annular, or in Fig. 11, in which bevel wheels are employed. But the binary structure of the complete epicyclic train, although disguised, still exists; in these latter figures, and in all similar cases, the wheels B, A, constitute one elementary pair, and B, F, another.

Now, considering only the relative motions of A, F, and T, it clearly makes no difference whether the train be composed of spur, bevel, skew, or screw wheels, in fact any means by which rotation is transmitted with a constant velocity ratio, may be employed without affecting the result. For the purpose of ascertaining the relations between these motions, then, we may take a train like that shown in Fig. 9, of which the essentials A, F, T, are represented in Fig. 12.

First, suppose T to be stationary, and let A be turned in a given time through any given number of degrees, m. Then F will in the same time be driven through some number of degrees, n, which can be found in the usual manner, since the whole train is now composed of wheels rotating about fixed axes; we shall then have for the value of the velocity ratio:

$$\frac{\text{ang. vel. A}}{\text{ang. vel. F}} = \frac{n}{m}.$$

Next imagine one of the wheels to be locked to the train arm, and let the latter, in the same time as that occupied in the previous motion, be turned through a number of degrees represented by a. Then the angular motions of both A and F, expressed in degrees, must be increased by just this number a; that is, if we represent their total motions by m^1 and n^1 respectively, we shall have, as shown in Fig. 13:

$$m^1 = m + a, \text{ or } m = m^1 - a,$$
$$n^1 = n + a, \text{ or } n = n^1 - a.$$

But these actions may take place simultaneously just as well as successively, and will do so if we unlock the wheels and set the train in motion.

We shall have then:

$$\frac{n^1 - a}{m^1 - a} = \frac{n}{m},$$

an equation expressing the relation between the three angular motions m^1, n^1, and a, from which, if any two of them be given, the third can be determined.

In deducing this expression, it must be observed, all the rotations, which have the same direction as those of the hands of a clock, are considered positive; a rotation in the opposite direction must therefore be regarded as negative, and due attention must be paid to this in applying the equation to any given train. Thus, if the train arm still turn to the right, but A or F turn to the left, m or n will be negative, and we shall have:

$$\frac{n^1 - a}{m^1 - a} = -\frac{n}{m};$$

If the train arm turn to the left, both A and F still turning to the right, a becomes negative, and

$$\frac{n^1 + a}{m^1 + a} = \frac{n}{m};$$

If the train arm, again, turn to the left, A and F turning opposite ways, we shall have:

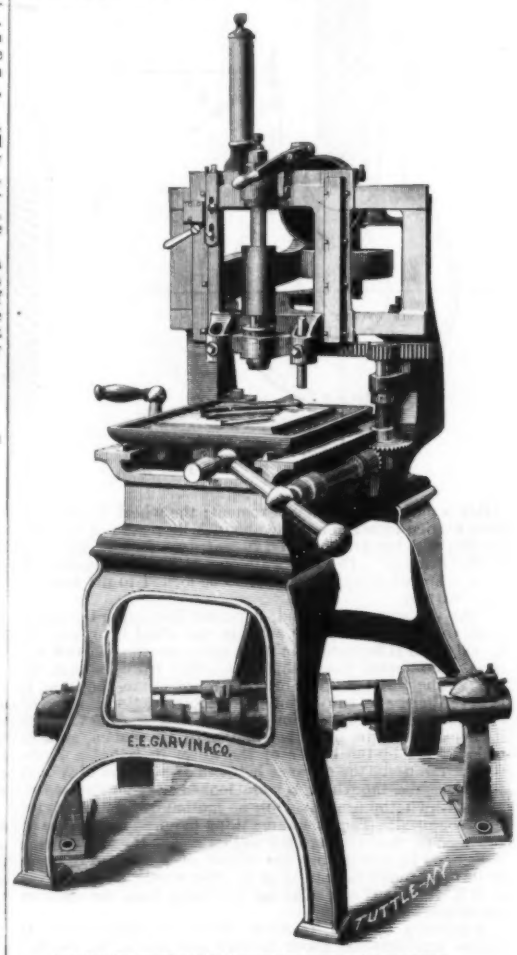
$$\frac{n^1 + a}{m^1 + a} = -\frac{n}{m}.$$

This is, then, the *general equation of the action of an epicyclic train*, as deduced by Prof. Rankine. And to its accuracy it is clear that no exception can be taken, when its application is confined to what we have called *complete* trains; that is to say, those in which the first and the last wheels of the entire train under consideration (as A and F in Figs. 9–13) are *both sun wheels*, having the same axis as that about which the train arm revolves.

Prof. Rankine expressly stipulates that this shall be the case (*Machinery and Mill Work*, p. 243); and he does not even suggest the application of this equation to *incomplete* trains; or in other words, the use of it to determine the axial rotation of any planet wheel of the train. Such an application has been made, however, by other writers, and in the next article we shall endeavor to show that the results are in some cases inconsistent with each other and directly at variance with acknowledged facts.

PROFILING MACHINE.

E. E. GARVIN & Co., of 139 & 141 Centre St., N. Y., have lately brought out an improved Single Spindle Profiling Machine, a tool presenting several points which merit special attention. Compared with the regular two-spindle style of edge-milling machine, it occupies but little more than half the floor space, being rather more compact on account of dispensing with the long drum, and in place of that, driving the spindle from a single pulley which keeps an equal belt tension, as it is held at the end of a radial arm, which preserves a constant center-distance between that and the center line of cutter-spindle.



SINGLE SPINDLE PROFILING MACHINE.

A special feature upon this machine is, that two former pins are used, fixed upon the line of and equally distant from the spindle center, allowing thereby a superior mode of placing the former and the work to be milled. When the former is to be cut from a sample piece, no resetting of the former is called for, as it is used in the exact position that it was made, and the work is milled upon the spot that the sample was fixed in making the former.

The four motions of traverse necessary, are accomplished by rack and pinion movements (adjustable) controlled from front end of machine by two large ball handles.

Four changes of speed are obtainable by means of a cone of two faces and a counter with a novel and superior clutch motion, allowing two speeds of shaft.

PROPOSED SUEZ SHIP RAILWAY.

THE proposal to transport ships overland, to secure a shorter and more direct route, is no new one. It was old even when Mr. James Brunlees, the President of the Institution of Civil Engineers, suggested a plan having this object, and to a certain extent elaborated its details. But it was not till Mr. James B. Eads occupied himself with the problem of carrying vessels and their cargoes across the Isthmus of Tehuantepec, from the Atlantic to the Pacific Ocean, that this problem was brought within measurable distance of solution. The weight of opinion, that is, of useful opinion, is in favor not only of the practicability, but of the certainty of carrying ships in this manner, uninjured and unstrained; work has been already actually commenced on the scheme, and we believe that Mr. Eads, whose well-earned reputation as an engineer is as great as his energy and capacity in finance, will undoubtedly, within a few years, connect the opposite coasts of the Panama Isthmus, and give a direct line of communication to vessels traveling by the east and west. Since Mr. Eads gave vitality to this scheme, at least one similar project has been seriously undertaken, and others proposed. Naturally the Isthmus of Suez suggests itself for such a work, because of the facility with which it could be carried out, the great importance of our interests in that work, and the possible danger which threatens those interests, and which still give rise to apprehension. Messrs. Clark and Standfield have recently exhibited at the London Chamber of Commerce drawings of such a scheme, and their drawings we reproduce from *Engineering* this week. Figs. 1 and 2 are respectively a side view and plan illustrating the railway. From these it will be seen to consist generally of a lift at each end, joined by a track illustrated by Fig. 3, traverse tables being introduced at intervals to allow of ships passing each other. Figs. 4 and 5 are enlarged views of the

PROPOSED SHIP RAILWAY ACROSS THE ISTHMUS OF SUEZ.

MESSRS. CLARK AND STANDFIELD, ENGINEERS, LONDON.

Fig. 1.

Fig. 2.

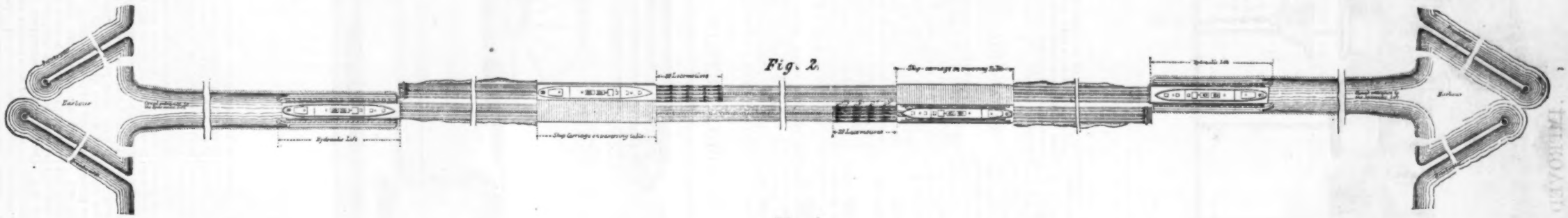


Fig. 3

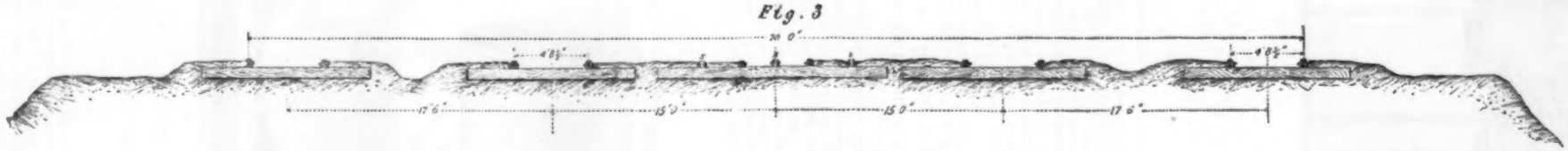


Fig. 4.

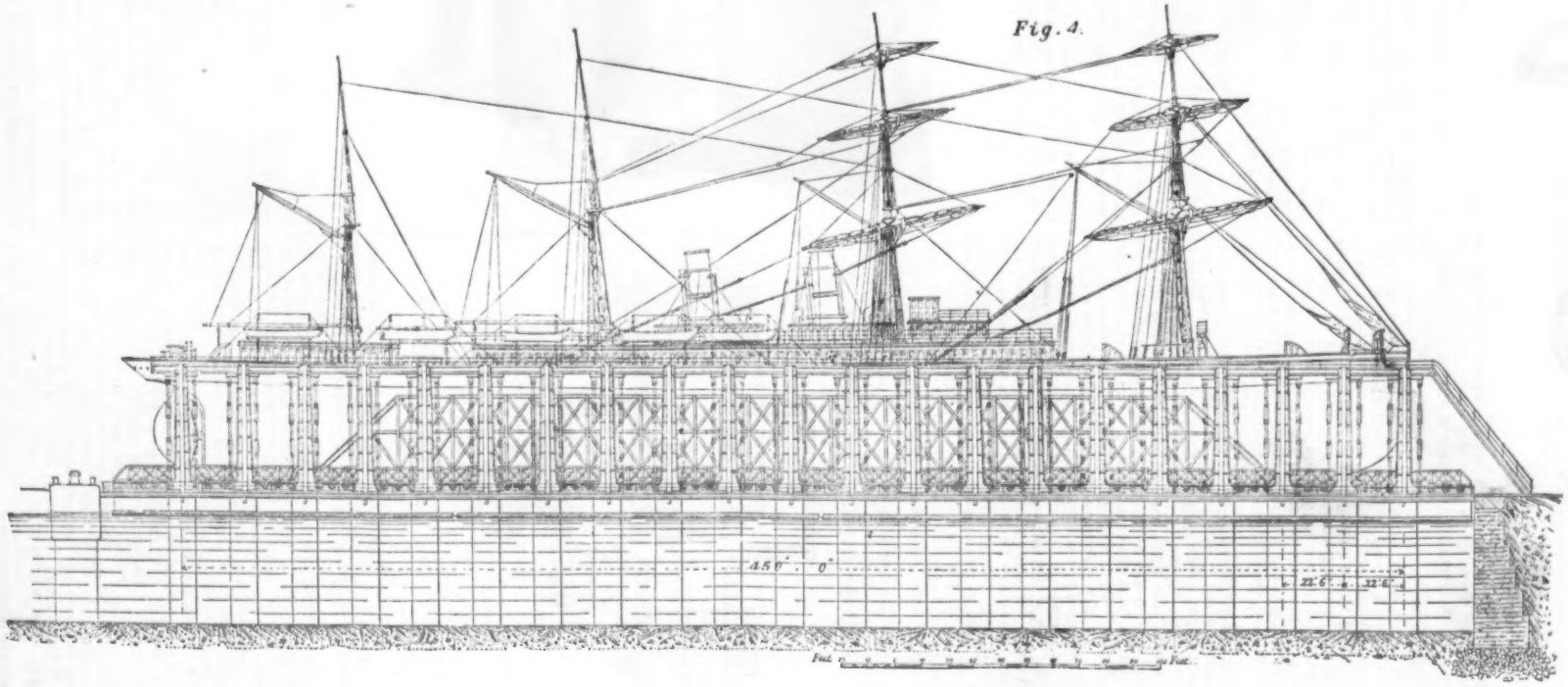
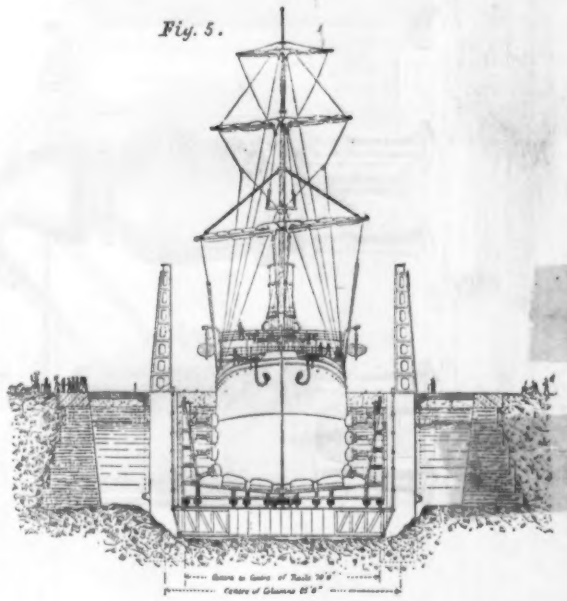
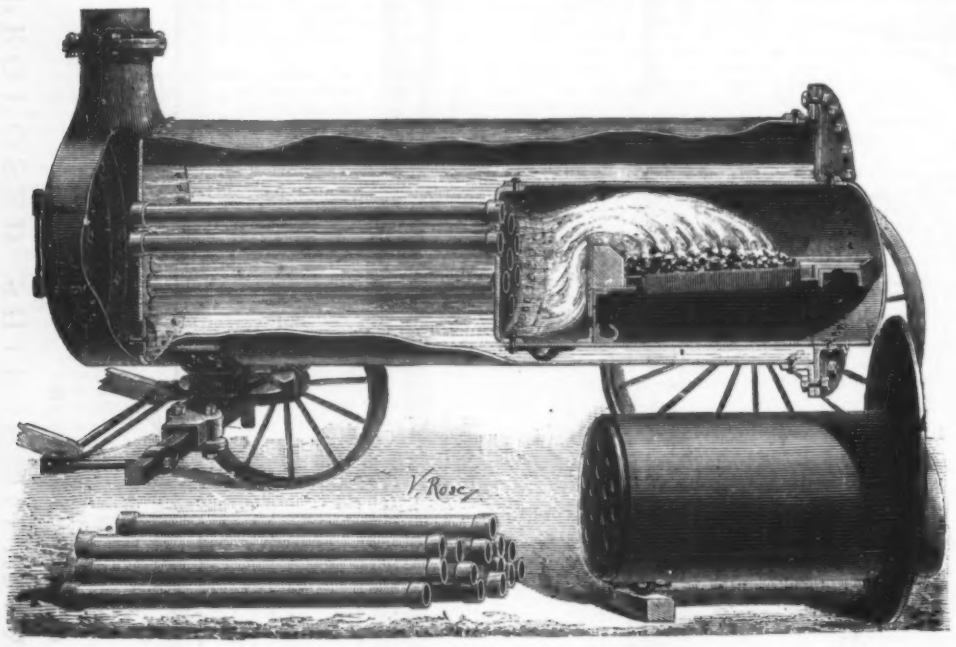


Fig. 5.



dock, the lift, the mode of supporting the ship, and the carriage upon which it is transported. In the lift on the right-hand side of Fig. 1 a vessel is shown about to be raised, while that on the left is elevated to the maximum, the ship being ready for transportation. The carriage, as will be seen, is provided on each side with four rows of Messrs. Clark and Standfield's air cushions, and four large ones are placed under the bottom of the ship. Those at the sides are controlled by rapidly worked struts, so that both narrow and broad vessels can be accommodated. Each of these rows of air cushions is divided vertically into ten separate compartments, giving a total of 120 compartments supporting the vessel all round; as these run at right angles to the frames of the vessel, she cannot be in the least strained, even if many of them were punctured. The pressure of these air cushions on the side of the vessel corresponds with the water pressure at the same heights when the vessel is at her load draught in smooth water. It would, by preference, be a little in excess of the water pressure, but would be very considerably less than the pressure to which the vessel is subject when laboring in a seaway. In the plan Fig. 2, are shown two traversing tables for transferring vessels from the up to the down line, or *vice versa*. These are placed near the lifts at each end of the railway, and worked by direct action hydraulic rams. To avoid curves sector tables are placed where required, to change the direction of the line.

The extreme breadth of the track is 70 ft. from outside rail to outside rail, see Fig. 5. It consists of five ordinary gauge lines of 4 ft. 8½ in., with the three supplementary middle rails, *s s s*, making thirteen rails in all. The gradients are very flat, the highest point on the proposed route being only 52 ft. above sea level, and it would be necessary to construct the line only about 8 ft. above that level. An ordinary hydraulic ship lift, such as those in use at the Victoria Docks, London, at Malta, and at Bombay, occupies from one to three hours in docking a vessel of from 2,000 to 6,000 tons weight; such a lift would take little more than three minutes to raise a laden vessel weighing 12,000 tons through a height of 40 ft., the maximum lift that would be required.



AUBER'S TUBULAR BOILER.

AUBER'S TUBULAR BOILER WITH MOVABLE FIRE-BOX.

MR. AUBER, a French manufacturer, has devised a system of portable motors that exhibit certain interesting improvements in the construction of the steam generator, such improvements having been introduced with a view of making it easy to get at the interior of the boiler so as to keep it in order. This is a very important matter, especially in agricultural engines, where the feed-water may make it necessary to clean the boiler quite frequently.

The mounting of the different parts of the generator and the form of the movable pieces may be seen by inspecting the figure.

The tubes, which are provided with conical ferrules, turned with care, are fastened into the tube plate in the usual way, that is to say, with a rod threaded at its extremity. This rod likewise serves to remove them in a few instants from the end toward the fire-box. It then only remains to remove the bolts from the front plate of the fire-box and disengage the latter from the tubular shell. All parts of the boiler are thus rendered accessible, and the cleaning of them may be effected under very satisfactory conditions. The fire-box carries a piece of corrugated iron plate which rests upon the bottom of the shell, and it is arranged in such a way as to give a large heating surface without, however, increasing the consumption of fuel. In generators of this kind the efficiency of a reversed flame is not as yet clearly established. Mr. Auber prefers to employ a tubular section of some extent, and a direct draught, this giving greater security. Upon reaching the smoke-box the sparks are extinguished before reaching the smoke-stack, and thus all danger of fire is avoided.

In short, this boiler combines within itself all those conditions of simplicity and strength that are requisite for agricultural engines, whose running is often intrusted to inexperienced persons.—*Revue Industrielle.*

NOTES ON PHARMACEUTICAL APPARATUS.*

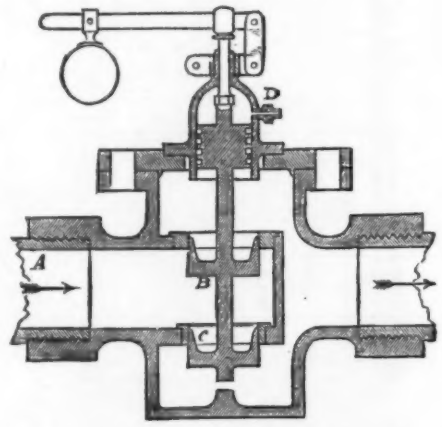
By CHARLES SYMES, Ph.D.

SOME years ago. Mr. Ince (I believe it was Mr. Ince) wrote thus, or to this effect: "I have a good deal of faith in a man and a pan, provided the one has brains and the other has capacity." These words forcibly conveyed to my mind that, in the opinion of the writer, a man of good average intelligence, with simple apparatus, sufficiently large for his purposes, could accomplish much in the way of manufacturing pharmaceutical preparations; and I think there can be little doubt that a pharmaceutical sermon, "having a man and a pan" for a text, could, in able hands, be rendered both interesting and instructive.

As a busy man, having my time pretty fully occupied with the duties of my calling, I merely propose to bring before you this evening some ideas concerning, and description of, a few simple but useful forms of pharmaceutical apparatus. I do so under the following circumstances:

Last year a friend from South America, when visiting Liverpool, desired to see our laboratory, about which he had probably formed some exalted notions; but on being shown the very modest equipment, after some conversation he said, "If so much practical work can be accomplished by such simple means, send me some similar things, such as your experience suggests as being necessary and useful to commence with for a small laboratory, and to which I can add as occasion requires." The work (except percolators, retorts, and minor matters) was, with rough drawings, placed in the hands of Messrs. Thomas Ryder & Co., engineers, of Manchester, and was by them executed in a highly satisfactory manner. I may add that as the apparatus had been shipped long before it occurred to me to write this communication, I am indebted to them for a photograph and some particulars to refresh my memory, and enable me to bring the matter before you in what I trust will be a clear and explicit manner.

I must mention that our friend had already a high pressure boiler working an engine and other machinery, and we had, therefore, to start with a reducing valve.



This valve is made of gun metal, and is so arranged on the steam supply main that it reduces therein from the pressure of the boiler, say 60 lb., to 2 or more pounds per square inch, as may be desired.

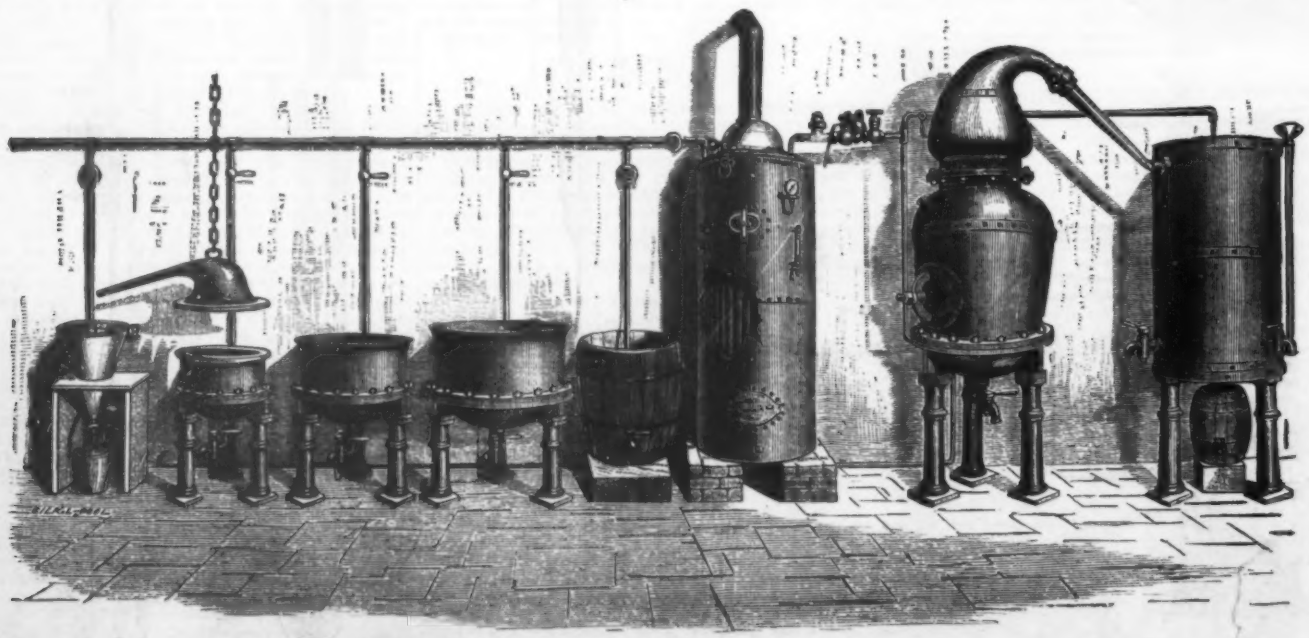
The steam entering at A, and acting equally on the two valve faces, B and C, it will be seen that there would be no passage but for the lever, which is weighted in such a manner as to allow just sufficient steam to pass through to produce the lower pressure and temperature. To the small union connection, marked D, is attached a brass tube to convey away any moisture from the upper chamber which may accumulate by the condensation of steam, as it is passing through the valve; and it also serves to prevent the possibility of a vacuum being formed which would hold the valves in check and prevent them from working freely.

As most pharmacists would provide a boiler specially for working such apparatus I am about to describe, let me say a few words about boilers, the choice of one being a matter of some importance. Our own is a small upright tubular, 4½ feet high by 26 inches diameter; it stands on nine inches of brick-work, and occupies but little space in the laboratory. An iron pipe, covered with felt to lessen condensation, runs from it round two walls of the building to supply steam to stills, pans, funnels, etc.

To have a boiler too small for the purposes required is a decided mistake; it is better to err in the opposite direction, as any one with experience in these matters will know how trying it is to run short of steam when two or three preparations are in hand, and a lot of evaporation has to be done. There is of course a limit in size, beyond which it is not desirable to go, first on account of space, and secondly on the score of economy in time and fuel, for if a much larger boiler is used than is really required there is so much more water to be heated each morning at the expense of these two items.

Boilers are usually described as of one or more "horse power," and to obtain this unit in the most simple form the following conditions are necessary, viz., 1 square foot

* Read at a meeting of the Liverpool Chemists' Association, February 28.



IMPROVED PHARMACEUTICAL APPARATUS.

of fire grate and 1 square yard of heating surface; these, with an expenditure of 14 lb. of coal, evaporate 1 cubic foot or rather more that six gallons of water per hour, which in the form of steam applied in a Boulton-Watt engine would perform the work of one horse for that period of time. A two horse boiler is sufficiently large where machinery is not used, and an upright tubular one is the best for "getting up steam" quickly. In this, coke should be used in preference to coal, as the latter is liable to choke the tubes with soot very readily. Although it should be tested to bear a pressure of at least 100 lb. to the square inch, not one-tenth this amount is generally used for pharmaceutical purposes.

With a low pressure boiler, then, a reducing valve is not required, and the steam is taken direct from it to the point at which it has to be used, merely interposing a screw stop valve on the main, which in such position is better than an ordinary tap, as it affords a more convenient means of regulating the supply. In the arrangement shown, a tub, three pans, and a funnel are placed on one side of the boiler; a still and condenser on the other side. Into the tub passes a block tin pipe, terminating on the bottom in a flat perforated coil, a short distance above which rests a perforated false bottom, such an arrangement being convenient for steaming and boiling purposes, where the ingredients are bulky, as in the case of decoctum sarzæ comp., etc.

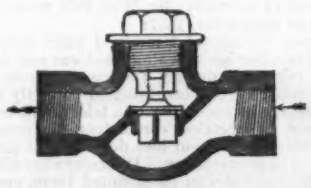
The pans are of copper, jacketed with cast iron, leaving a steam cavity over the lower part only. This provides sufficient heating surface for rapid evaporation, and avoids to some extent the inconvenience and possible deterioration of the product caused by the drying which always occurs on the outer edge of the liquid where it comes in contact with the sides of the pan when it is jacketed to the top. Constant stirring will of course obviate this to some extent, but even with this, and the old arrangement, it is not easy to remove the difficulty.

The capacities are 40, 20, and 10 gallons, and the internal measurement, 30×24, 24×20, and 18×17 in. respectively, and such a one is fitted with a check valve, or tap, vacuum valve, and a siphon pipe and back pressure valve at the outlet for waste steam; it is self-contained, requiring only the attachment of the steam supply and waste pipes, so that, if thought desirable, the relative positions could be changed with ease. The steam funnel is made entirely of copper, well tinned inside, and the steam chamber is continued to the top. The steam supply enters at a ¾ inch union near the top, makes the circuit of the chamber, and passes out at right angles to the side of the funnel, more or less condensed, near the bottom. The exit tap is somewhat large in proportion to the funnel itself, the object of so having it being to prevent any possibility of choking up and to provide a good exit for any thick or partly solid filtrate. The interior of the funnel is fitted with a tinned wire cagework which can be removed at will, its obvious use being to prevent the filtering material from touching the sides of the funnel, and for preserving a clear passage for the filtrate in all directions, while the support given is practically equal to that by a solid surface. Near the top is placed a small safety valve which is regulated to blow off at 5 lb. pressure. The main steam pipe (if not required for drying closet) should terminate in this direction with a T-piece, fitted with screw tap and hose connection, so that a rubber tube can be attached when desired; it is useful for many purposes; one example is that of steaming nux vomica beans before grinding.

The still is placed on the opposite side of the boiler; it is of copper with a cast iron jacket, similar to those on the pans, and has fittings like them. In the body of the still is a gun-metal hand-hole for removing any solid residue, and for cleaning out purposes. It is also fitted with a removable perforated false bottom for separating any solid matter from the liquid contents drawn off by the tap underneath. The distillate is condensed in a block tin worm contained in a circular wrought iron cistern of 60 gallons capacity, the exit being from the side near the bottom.

Passing direct from the boiler is a steam-pipe which terminates in another block tin worm at the top of the cistern, going within the coil of the other worm and out at the center of the bottom of the cistern. Underneath is a stoneware barrel to receive the distilled water as it flows from from the last-named worm. This is found to be a convenient arrangement, and whenever the pressure is increasing unduly round the still or pans, or when it is necessary to stop the working of one or more of these, then the distilled water tap can be turned on, with the result that one has a good supply of distilled water, practically free of cost.

It has been mentioned that each pan is fitted with a siphon pipe and back pressure valve. The former prevents waste of steam, and consequent power; a foot of condensed water in the pipe would represent approximately half a pound of steam pressure. The latter prevents the waste steam from one pan from entering the chamber of the next, replacing a tap and acting automatically; it is exceedingly simple, as will be seen from the drawing. The arrows indicate the direction in which the steam (or condensed water) passes, lifting the small valve in the center and moving forward freely; but if it attempts to pass in the opposite direction, the more tightly the valve is closed against it.

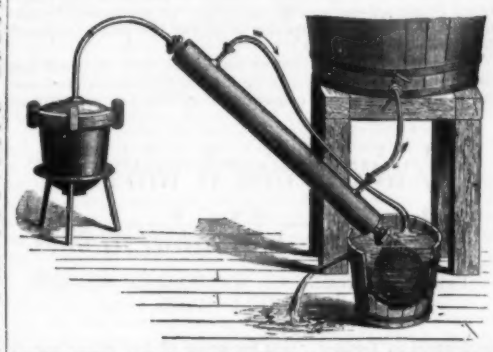


The apparatus described does not by any means constitute the equipment of a pharmaceutical laboratory, but it forms some of the essentials for every day work, and a foundation on which to build. The capabilities are but moderate, still the expense is moderate also. The cost, exclusive of main supply and waste steam pipes, the tub and stone barrel, is £110. A two horse upright tubular boiler costs £28 or £30, so that the whole could be fitted up for something like £150. The still, pans, and funnel are tested by hydraulic pressure to bear 20 lb. on each square inch of surface.

Doubtless, different pharmacists work their apparatus at different degrees of pressure. My experience is that with pans of the size mentioned a pressure of from 2 lb. to 5 lb. to the square inch, giving a temperature of from 216° to 235½° F., is the most suitable when stirring is actively continued, and when not stirring, a nominal pressure say of ½ lb. to the square inch is quite ample.—*Pharmaceutical Journal.*

RETORT AND CONDENSER.

WE give an engraving of a retort and condenser for gold amalgam. The retort is turned smooth inside, and is of a shape peculiarly adapted for rapid and complete exhalation of the quicksilver. Of course this can be used for silver amalgam if desired.



IMPROVED AMALGAM RETORT AND CONDENSER.

The condenser attachment, shown in the cut, which is made of either brass or iron, is, as will be observed, of very important use in facilitating the process of retorting by decreasing the time required, and permitting of a more perfect condensation of the quicksilver through its automatic action; adding but little to the cost, and can be applied to any retort pipe, as it is not a fixture, and if made of brass will not rust. The rubber bag attached to the end of the retort pipe prevents all danger of explosion. It should, of course, be immersed in a vessel containing water. The distention of the bag indicates that quicksilver is still being condensed, and its collapse indicates that the operation is complete. The Hendy machine works have these appliances. The tripod or retort stand can be made by any blacksmith.—*Min. and Sci. Press.*

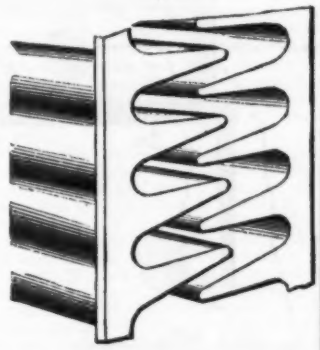
LAWRENCE'S CAPILLARY COOLERS.

THE Messrs. Lawrence's system of coolers, represented in the accompanying engravings, are already well known to brewers and distillers, and are particularly interesting from the facility with which their principle adapts itself to the construction of surface condensers for steam engines. They are, on another hand, employed with success for the cooling of wort, for the condensation of alcoholic vapors, and for the treatment of spirits, wine, milk, and other liquids.

Fig. 1 gives an ideal view of the essential part of the capillary cooler, and Fig. 2 gives the actual form of the

FIG. 1.

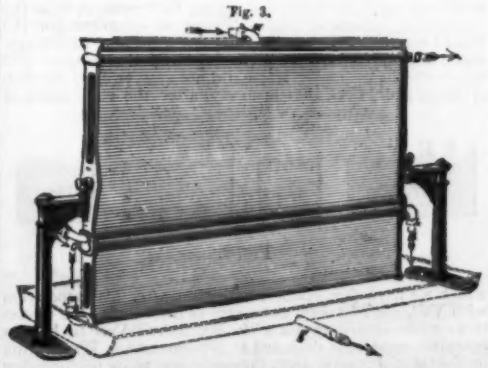
Fig. 2.



undulations. Two plates of tinned copper are placed near each other, and are so bent as to form a series of corrugations over which the liquids circulate in opposite directions—the water rising in the interior in the directions shown by the arrows, E, and the liquid to be cooled passing over the undulations in the direction, L. These two currents have naturally ingress and egress orifices alternately at the top and bottom of the apparatus, and the latter is protected from the air by well jointed side plates.

The conductivity of the metal, its thinness, and its undulatory form, and the circulation of the liquids in opposite directions, lead to a very effective refrigeration. The apparatus shown in Fig. 3 has been specially devised for the manu-

Fig. 3.



LAWRENCE'S CAPILLARY COOLER.

facture of beer of low fermentation. It acts upon the wort by means of ordinary water and iced water. There are other single-liquid types which serve for cooling beers of high fermentation.

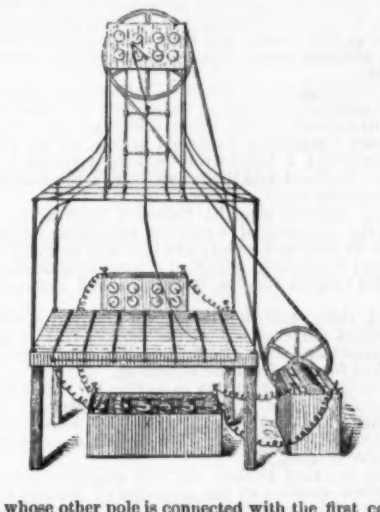
In Fig. 3, A shows the entrance for the iced water, B its place of exit, C the entrance for the ordinary water, D its place of exit, E the entrance for the wort, and F its place of exit.

The apparatus are all provided with a copper receiver, a pipe for distributing the liquid to be cooled, and sufficient surface of convolutions to cool thirty hectoliters per hour to one degree above that of the cooling liquid.—*Revue Industrielle.*

THE PLAY OF THE NEEDLE.

THE accompanying cut represents a machine designed to calculate the number π based upon the play of the needle.

It consists essentially of a wooden table, covered with an insulating varnish, upon which are placed copper bands. These latter are separated from one another by an interval of a few millimeters, in the center of which are arranged copper wires that project slightly and are insulated from the copper bands. All the copper plates are connected with each other through the intermedium of the conductor, and all the wires are connected with each other. As shown in the figure, the conductor communicates with the pole of a



pile whose other pole is connected with the first conductor after passing through an electro-magnet in front of which is placed a fork. Ordinarily the circuit of the pile is open, and the apparatus is at rest; but, if a metallic needle be thrown upon the table so that it shall rest at the same time upon a band and a wire, the current will pass and the iron plate of the fork will be attracted. When the needle is raised, the plate will fall and the wheel will have revolved one notch.

Let us imagine, then, that a counter is connected with the wheel, and we shall see that every time the needle meets a copper wire the counter will mark one.

This granted, we connect the needle, by a fine and very flexible wire, with a rod set in motion, through the intermedium of a connecting-rod, by any motor whatever, an electric one, for example, as shown in the figure. A second counter, placed at the top of the apparatus, indicates the number of times the needle has been lifted and then thrown upon the table.

If then we suppose that the needle is just equal to half the interval comprised between two of the copper wires upon the table, we shall have at every instant a value of π in the form of a fraction whose numerator will be marked by the upper counter and whose denominator will be shown by the lower one.

Let us remark finally, that, every time the lower counter marks a power of 10, the upper one will give π as a decimal fraction with as many decimal figures as there are units in the power of 10 in question.

It will suffice, then, to put in the counter as many wheels as it is desired to have decimal figures, in order to have π with any approximation whatever.—*J. Lecornu, in La Nature.*

THE GREAT DISCOVERIES IN ELECTRICITY.*

FARADAY was the first, as long ago as 1821, to cause a magnet to revolve through the action of a current. Ten years afterward, in 1831, he discovered induction currents under the following circumstances:

The influence that a body charged with ordinary electricity exerts upon the electrical state of another body placed near it, had been known for a long time. The connecting wire of a pile in which a current is circulating is evidently an electrified body. Is this wire then capable of modifying the electrical state of a second wire placed near it, and of exciting a current in it?

All attempts that had been made to answer this question having failed, Faraday took up the study of it. He formed, in the first place, a helix consisting of two insulated wires, which he wound parallel with each other, one over the other, around a wooden cylinder. The two ends of one of the wires communicated with a very sensitive galvanometer. In the other wire circulated the current from a pile whose power Faraday kept progressively increasing by raising the number of its elements from 10 up to 120. In spite of this the galvanometer showed no change in the electrical state of the other wire during the continuous passage of the current. But, fortunately, Faraday perceived that at the moment at which he closed the latter, the needle of the galvanometer experienced a sudden and momentary deflection, and then returned to a state of rest in spite of the continuity of the current.

He saw also that at the instant he broke the latter the needle underwent another sudden deflection, but one that was in a direction contrary to the other. He concluded from the circumstances connected with this unexpected phenomenon that when the current from the pile begins, there exists in the neighboring wire an instantaneous or *induced* current, which has a direction opposite that of the exciting or *inducing* one, and that at the moment the latter ends an induced current is excited anew in the neighboring wire, but in the same direction as the inducing current. He perceived also that a current which is rapidly approaching a closed circuit excites therein an induced current, in acting like one that is commencing, and that, as soon as the first leaves, it acts like one that is ending.

It was thus that Faraday discovered instantaneous where he thought he would find permanent induction, like those who had already occupied themselves with this question.

*See SCIENTIFIC AMERICAN, July 14, page 25.

It remained to find *electro-magnetic* induction, or the mode of production of electricity by magnets—a problem the same as that which Ampere had solved ten years previously.

Faraday wound separately, around a soft iron ring, two perfectly insulated wires, one of which was in connection with a pile and the other with a galvanometer. At the moment he closed the circuit the ring became suddenly magnetized and the galvanometer needle revolved quickly and then came to a state of rest. This deflection revealed the instantaneous excitation of an induced current that was developed in the wire of the galvanometer by the sudden magnetization of the iron ring.

When Faraday broke the current this ring lost its magnetism and the needle at once showed a momentary deflection. This was due to the formation of a new induction current, through the demagnetization of the ring.

Faraday obtained similar effects when he quickly introduced into a hollow, single wire bobbin, communicating with the galvanometer, either a bar of soft iron magnetized by a strong current, or a powerful permanent magnet. In both cases the galvanometer needle was deflected quickly in one direction at this instant, and then in an opposite direction when he suddenly removed the magnets from the hollow in the bobbin.

These experiments on induction by magnets, which are the most surprising, belong to the domain of those on induction by currents, if the theory according to which Ampere likened a magnet to a solenoid be true, for the magnet thus represented a bobbin traversed by a voltaic current that was introduced into the hollow bobbin communicating with the galvanometer.

Faraday likewise obtained induction currents in wires through the influence of terrestrial magnetism circulating from east to west, and we may add here that he found in the phenomena of magnetic induction a true explanation of the magnetism through rotation that Arago had discovered in 1824.

In 1834, three years after these great discoveries, Faraday demonstrated the existence of the *extra current*, or the induced current that occurs in the inducting circuit itself, and that he had foreseen from the beginning.

He studied this phenomenon in seeking the reason for the appearance of a spark of a peculiar character, and of the strong shock that Jenkin and Pouillet had experienced at the moment of the breaking of a current circulating in a large electro-magnet.

Faraday hastened to show that the electricity developed by induction possesses all the properties of ordinary electricity produced by machines or the pile, and to prove thus that induction is, in reality, only a peculiar mode of excitation of electric manifestations.

The discovery of electro-magnetic induction, while it has thrown a new light upon the relations existing between magnets and electric currents, has given us a means of producing electricity with more facility and advantage, as may be seen from the machines of Pixii, Clarke, Ruhmkorff, Siemens, Gramme, De Meritens, and others.

Some of these serve for producing the electric light, etc., while others constitute powerful magneto-electric or dynamo-electric machines that have recently been applied to the conversion of mechanical force into electricity, and, by a reverse action, electricity into mechanical power.

The first magneto-electric machine was constructed toward 1850, at Paris, by F. Nollet, professor of physics at the Brussels Military School.

If we cast a rapid glance over the progress that has been made in electricity in less than half a century, we shall see that, after presenting itself suddenly under an entirely new form in the volta pile, after manifesting its surprising action upon magnets and currents at a moment when the science of the phenomena engendered by the pile seemed to some to be exhausted, electricity is again presenting itself under a new light in induction apparatus, wherein it is excited by the most surprising means.—*Ch. Montigny, in L'Electricite.*

[Continued from SUPPLEMENT No. 435, page 6786.]

HISTORY OF THE ELECTRIC TELEGRAPH.

AMONG the telegraphs that are based upon the use of the electro-magnet we shall first speak of Morse's, by reason of the date of 1832 that some have desired to assign to it. In order to well understand at what epoch the Morse telegraph was really invented, we must go back a little in the life of the inventor. Samuel Finley Breese Morse, born at Charlestown, in 1791, was an historical painter. From 1811 to 1815 he was in Europe improving himself in the art of painting; then he returned to America. In 1829 he again came to Europe, which he left in the packet ship Sully, in 1832, for his own country again. Upon the same vessel was Dr. Charles T. Jackson, of Boston, who had been attending the lectures of Pouillet upon electro-magnets at Paris, and who had among his baggage a pile and an electro-magnet of Pixii's make. The wonderful effects that Jackson had seen produced with this apparatus at Pouillet's lectures soon became a subject of conversation among the passengers, and Morse, who had never specially occupied himself with electricity, but who had, however, during the winter of 1826-1827, taken lessons of Dana, at New York, upon electricity, took a lively interest therein. The conversation several times turned upon the advantage that might be derived from the electro-magnet, and Jackson, as well as Morse, spoke of the possibility of employing it in the construction of a telegraph. The end of the voyage came, and Morse and Jackson each returned to his occupation. Jackson thought no more of the telegraph, but, as for Morse, the idea of constructing one was ever in his thoughts. According to Turnbull, he was, in 1833, observed in the house of his brother, in New York, casting characters designed for the production of dots and dashes. Then his researches seem to have been interrupted until 1835, the epoch at which he was appointed professor of the history of the arts of design in the University of New York, and was located in the University building. It was here, it appears, that he constructed an apparatus in conformity with his first ideas, and exhibited it (according to his declaration before a justice) to a few friends, among whom was the chemist Leonard Gale. But the apparatus did not as yet give a favorable result, and, if it did better a short time afterward, it was doubtless because Morse had been aided by the advice and scientific knowledge of Gale, and perhaps too because he had obtained some knowledge of what was being done in Europe. However this may be, on the 4th of September, 1837, he addressed to the New York *Journal of Commerce* (which a few days before had for the first time spoken of his telegraph) the following letter:

"Gentlemen: A few days ago you had the kindness to support my rights to the invention of the electro-magnetic telegraph, and I thank you for it. I can give the most detailed proofs as to the priority of the invention; it is now five years ago that I invented and almost entirely carried out my plan. You have announced that I was preparing a circuit of short extent in order to show my friends the operation of the telegraph. This circuit I made about a third of a mile long, and last Saturday (the 2d instant), in the presence of Professors Gale and Torrey, of this city, and of Prof. Danberry, of the University of Oxford (Eng.), and a few other persons, I made a preliminary experiment with the register. The dispatch was written perfectly enough to establish the possibility of the scheme and the simplicity of my mode of communication, which is superior to that of the plans proposed by European professors.

"It will be observed that no foreign electric telegraph has as yet succeeded in transmitting intelligible communications; it is only asserted, as a consequence of the last experiment

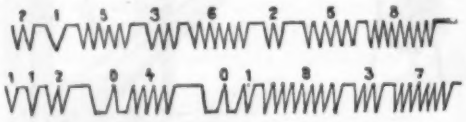


FIG. 1.—SPECIMEN OF WRITING OF MORSE'S FIRST TELEGRAPH.

performed in London, 'that by means of five wires,' etc., a dispatch '*may* be transmitted.' I send you a specimen of the writing of my telegraph. It is the transmission of a dispatch (effected this morning in a completer manner than on Saturday) to a distance of a third of a mile. Thinking that your readers would be pleased to see this kind of writing, I have had it engraved for you, and subjoin an explanation of it."

The specimen of writing is shown in Fig. 1. It comprises the numbers 215, 36, 2, 58, 112, 04, 0, 1837, which, on being looked for in the dictionary, gave the sentence: "Successful experiment with telegraph, September 4, 1837."

This letter well shows that Morse had not before this obtained any satisfactory result. On the 2d of September,

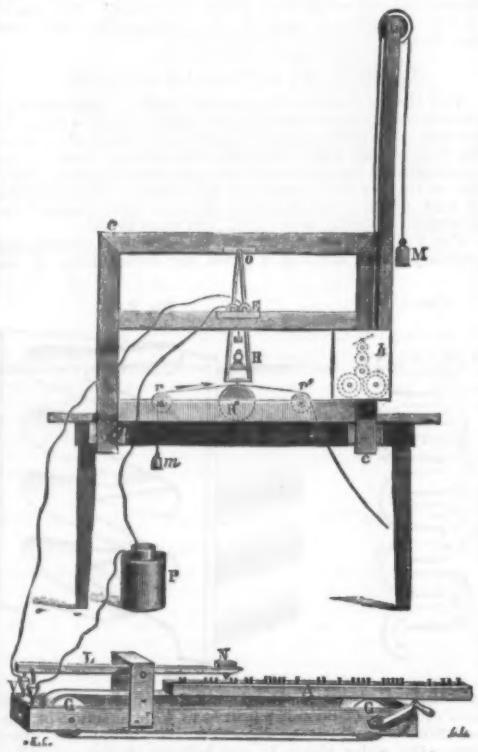


FIG. 2.—MORSE'S FIRST RECEIVER.

even, the transmisaion was but incomplete; it was only on the 4th that there was success, and the very tenor of the dispatch carries upon the face of it the evidence of a first triumph. Moreover, according to Vail, the constructor, who soon became Morse's associate, the exhibition on the 2d was the first public experiment with the telegraph.

On another hand, the Secretary of the Treasury had, on the 16th of March, 1837, sent to a large number of persons, Morse among them, a circular asking their opinion as to the "possibility of establishing a system of telegraph for the United States," and as to the different ways that such a system could be rendered more useful to the Government and the public in general. Morse did not answer until the 27th of September, 1837, that is to say, after the experiment of

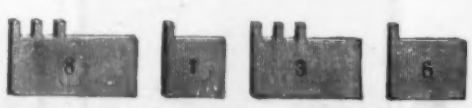


FIG. 3.

which we have just spoken, and even after his association with Vail, after having, as he says in his letter to the Secretary, made arrangements with Mr. Alfred Vail to have a complete apparatus designed to demonstrate at Washington on the 1st of January, 1838, the possibility of its performance and the superiority of his mode of telegraphic communication by means of electro-magnetism.

Finally, it was not till October 6, 1837, that he made application in America for a patent, an application, be it said *en passant*, that he soon withdrew so as not to be hampered in the taking out of patents in Europe.

Despite all these facts, which accord so well to show that his telegraph did not really work till 1837, Morse did not hesitate to claim 1832 as the date of his invention. In his first letter to the Secretary of the Treasury he expresses himself as follows:

"About five years ago, on a trip from New York to Europe, Franklin's experiment with a wire nearly four miles long came to my mind in a conversation with one of the passengers. This experiment proved that electricity accomplishes its circuit within an inappreciable length of time, and apparently instantaneously.

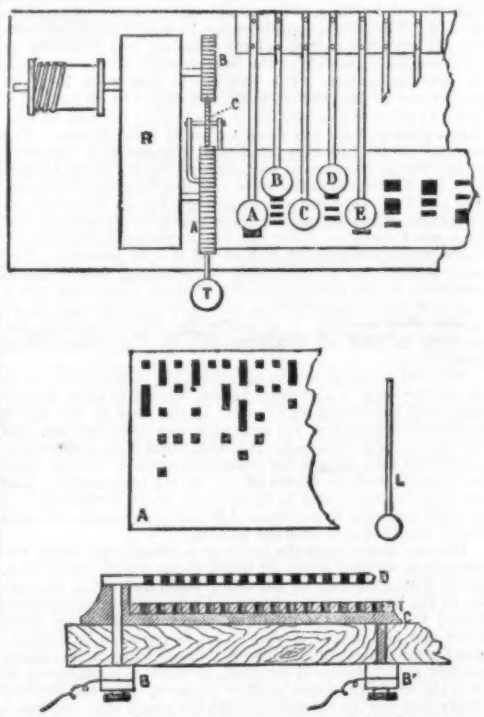
"It also occurred to me that if the presence of electricity could be rendered *visible* in any part of the circuit whatever, it would not be difficult to construct a *system of signals* through which a dispatch would be transmitted instantaneously. This idea grew in my mind during my entire trip, and I devised a system of signs and an apparatus to put it in execution. The first week after my return I established the base of my system. The result exceeded my expectations. . . ."

In another letter, whose destination is not indicated, he expresses himself still more clearly:

"In 1826, the lessons given at the New York Athenæum by my intimate friend, Dr. J. F. Dana, gave me the first notions of electro-magnetism, and made known to me some of the properties of the electro-magnet; and I made use of these in 1832 for establishing the bases of my plan of an electric telegraph. I claim to have invented the electro-magnetic telegraph on the 19th of October, 1832, on board the packet ship Sully, during my trip from France to the United States; *consequently*, I am the inventor of the *first truly practical telegraph based upon electrical principles*. The plan that I then conceived with all its essential characters is that which is now in active use. All the practicable European telegraphs are based upon a different principle, and *without a single exception*, postdate my own.

"I should have perhaps said that the conception of the idea of an *electric telegraph* belonged to me then, and that I believed myself to be the first who had ever associated these two words.

"It was not till after the realization of my invention, and its successful trial over a length of ten miles, that I learned for the first time that the idea of an electric telegraph had occurred to some one else. I can say that my idea belongs to me alone from its total dissimilarity to the inventions and suggestions, even, of others. I had not the least knowledge of what had been done before me until my plan was in full operation. . . ."



FIGS. 4 AND 5.—MORSE'S FIRST TRANSMITTERS.

Morse strengthened his claims by letters from persons who had made the trip with him on the Sully. One of the passengers, Mr. W. C. Rives, wrote him on the 21st of September, 1837:

"Sir: I hope you will find in my numerous engagements an excuse for the dilatoriness that I have displayed in answering you upon the subject of your electro-magnetic telegraph. I remember perfectly the explanation that you gave me of this ingenious invention during our voyage to the United States in 1832. We often talked of it, and I raised difficulties that you promptly solved."

The captain of the Sully, Mr. Wm. Pell, wrote in his turn on the 27th of September, 1837:

"Sir: Upon my arrival I received your letter in which you ask my recollection touching what was said in regard to an electric telegraph during the passage from Havre on board the Sully, in October, 1832. I perfectly recall your idea concerning the possibility of a telegraphic communication by means of electrical conductors. In measure as the voyage progressed and your idea developed, it became a frequent subject of conversation. Difficulty after difficulty was suggested to you, but you surmounted them, and your invention, passing from a crude to a perfect state, through different degrees of improvement, seemed to have reached the state of an instrument that needed but patronage alone to cause its action. I am in sincere hope that untoward circumstances will not deprive you of the reward that is due an invention which, whatever be its source in Europe, belongs at least properly to you.

"When, on leaving my vessel, you said to me, 'Captain, when you hear my telegraph spoken of as the wonder of the world, remember that the discovery of it was made on board the ship Sully,' I little thought then that I would ever be called upon to strengthen by my testimony your rights to the priority of an invention that seemed so astounding a novelty."

A little later on, on the 1st of February, 1838, after seeing the telegraph operate at the University, Pell wrote:

"When, a few days ago, I was examining your apparatus, I recognized therein the principles and mechanical arrangements that I had so often seen you develop on board."

But, on another hand, at the session of March 4, 1839, Mr. Elie De Beaumont, a member of the Academy of Sciences, received from Dr. Jackson a letter in which occurred the following passage:

"I regret to see in the public papers that Prof. S. F. B. Morse has appropriated my electro-magnetic telegraph to himself. I explained this apparatus to him at length on board the packet ship Sully, while I was returning to America, in the month of October, 1832. I am pained at the unmerited patronage that French scientists have accorded to Mr. Morse. The invention that he has shown them belongs entirely to me. As soon as I knew what his claims upon this subject were, I addressed my protest to him, but I see that he is persevering. I beg you to inform the Academy that Mr. Morse did not invent the new telegraph, and that I gave him the description of it in October, 1832."

In the presence of assertions so contradictory, it is impossible to attribute any value to what occurred upon the Sully in 1833. We do not know what confidence to put in Dr. Jackson's assertion, but, in return, we are led also to suspect the good faith of Morse when we see, in a letter dated December 6, 1842, and consequently written at a time when, after a new trip to Europe, he must have been well informed, the following resumption of the history of the electric telegraph: "Ampere's idea is without doubt the source of all the more or less fortunate attempts made in Europe to construct an electric telegraph. In this category may be ranked the plan of Baron Schilling, of St. Petersburg, consisting of 36 magnetized needles and more than 60 metallic conductors—a plan devised, it appears, at the same time that I invented my electro-magnetic telegraph in the fall of 1832; the plans of Professors Gauss and Weber, of Gottingen, in 1833, which simplified the use of the preceding by the use of but one needle and but one current; that of Professor Wheatstone, of London, in 1837, which employs 5 needles and 6 conductors; and that of Professor Steinheil, of Munich, in 1837, which employs 2 needles and 2 conductors. "Professor Wheatstone announces that he has recently (in 1840) invented an electro-magnetic telegraph different from his invention of 1837. . . . This telegraph is, then, the first European electro-magnetic one, and it was invented, as may be seen, eight years after mine, and one year after the latter had been exhibited in public at the residence of Count Lincoln in London."

The error relative to Schilling, and the silence that Morse preserves upon the subject of Steinheil's ground wire, can scarcely be attributed to ignorance, but seems rather to be voluntary. Moreover, is not the true date of an invention that of its first publication and of its first being set in operation, say as regards the invention under consideration, 1837?

At this period the receiver of the Morse apparatus (Fig. 2) was made out of an old stretching frame, *cc*, nailed vertically against the side of a table. A transverse bar supported an electro-magnet, E, whose armature was fixed to a sort of pendulum, OB, that carried at its lower extremity a pencil which normally traced a straight line upon a paper band, *rr'*, that unwound under the influence of a clockwork movement, *h*; when the armature was attracted during the motion of the paper, and then suddenly detached, a V-shaped character was obtained. By combining the attractions and repulsions of the armature, writing was obtained similar to that shown in Fig. 1. The signs transmitted were numbers that corresponded to words inscribed in a dictionary. A single V denoted the figure 1, two V's in conjunction denoted 2, etc. The groups of V forming the figures were separated by short intervals, and the groups of figures forming a number by wider intervals. When the numbers, instead of corresponding to the words in the dictionary, were to be read as numbers, they were preceded by an inverted V that signified zero and indicated that figures properly so called were being indicated.

In order to produce this zigzag writing the transmitter shown at the lower part of Fig. 2 was used. A lever, L, lifted by a counterpoise, N, closed the circuit by causing a metallic bridge to plunge into two mercury cups, V, the depressions of the lever were produced by means of a device, A, which, under the influence of a winch, and of a ribbon stretched over two pulleys, C, passed under a point arranged at N. This device contained leaden characters (Fig. 3), whose notches lifted the point and closed the circuit as many times as there were zigzags in the character to be transmitted.

Shortly after the first public experiments made with this apparatus, the electro-magnetic telegraph was presented before the Senate and House of Representatives of the United States, and a committee was appointed to study it.

This same year (1838) Morse came to France, where he had his invention patented, and where, on the 10th of September, Arago presented his apparatus before the Academy of Sciences. Morse was equally well received by Mr. Alphonse Foy, the director-general of telegraphs, who gave him assurance of support in case experiments should be made with electric telegraphs. Afterward he went to England, but there Wheatstone and E. Davy, who already had patents for electric telegraphs, made opposition to the request that he had made for a patent, and so the latter was refused him.

In 1839 Morse returned to America, after Baron De Meyendorff had given him cause to hope that his telegraph would be adopted by the Russian government. He was deceived in this, however, and the disappointment he experienced caused him, he said, to stop for a while in the pursuit of his enterprise. He soon set himself to work again, however, and occupied himself with different improvements, and studied, among other things, how the number of pile elements was to be increased when the length of the line was increased.

In 1842 a report of the committee appointed to study the Morse telegraph proposed an appropriation of $30,000 for the construction of a line of sufficient extent to fully prove the possibility and utility of the system. This sum was voted on the 3d of March, 1843, the line was established between Washington and Baltimore, over a length of forty miles, and, on June 4, 1844, the Secretary of the Treasury, in transmitting Morse's report upon the experiment, asserted that the perfect possibility of the system was from thence strictly demonstrated.

At this epoch important improvements had been introduced into the apparatus by its inventor. The zigzags had been, previous to 1840, replaced by dots and dashes. The transmitter had passed through successive modifications, and the characters had been placed in a hopper in the order desired, and led one by one to act upon the contact lever. In another arrangement there was, for each letter, a wheelwork actuated by a key. By pressing upon the latter the wheelwork was wound up, and then the key, returning to its initial position, effected the closings and openings of the circuit necessary to transmit the character.

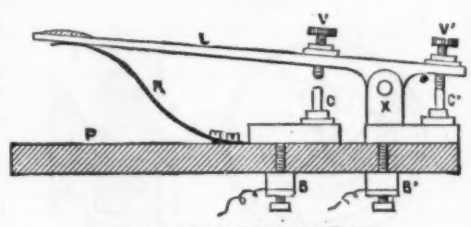


FIG. 7.—MORSE TRANSMITTER.

In a third transmitter (Fig. 4) a train of clockwork, R, set in motion a toothed wheel, B, and a second toothed wheel, C, mounted upon a forked lever, C T, permitted of communicating motion at will to the toothed wheel, A, of a long cylinder. This latter, which was of brass, had its entire surface, with the exception of certain groups of metallic projections (shown in black in the figure), covered with an insulating material. Springs that corresponded to each letter, and that rested against the cylinder through the intermedium of a metallic roller, all communicated with one of the wires of the circuit, while the metal of the cylinder communicated with the other wire. When with one hand the lever, T, and with the other one of the springs was depressed, the cylinder began to revolve, and the projections, passing under the spring, produced the necessary contacts for transmitting the corresponding letter.

Another, called the flat transmitter, was, so to speak, merely a simplification of the preceding. A plate of copper, C (Fig. 5), was provided with projections separated by an insulator in such a way as to form a bed, I, which in plan appeared as shown at A. Each vertical line of long and short projections corresponded to a character. Above the plate there was another one, D, provided with a long slit over each character. The plate, D, was connected with one of the wires of the circuit, and C with the other. In order to transmit a letter it was only necessary to introduce into the slit corresponding to this character a metallic rod, L, that pressed at the same time against A and the edge of the slit. Thus were the necessary contacts and interruptions obtained.

As for the apparatus employed during the experiments between Washington and Baltimore, this is shown in Fig. 6. In this instrument, which makes an approach toward the present Morse apparatus, the electro-magnet, HH, in attracting its armature, lifted toward the band of paper carried along by wheelwork, R', the extremity of the lever, T. This extremity was provided with several points that traced upon the paper dots and dashes according to the duration of the attraction. When the apparatus was not operating, a small vertical rod, fixed to the lever, T, caused a brake to bear upon a wooden wheel. At the moment the armature was attracted, this rod rose with the lever and ungeared the wheelwork, so that the paper unwound only during the transmission of a dispatch. The transmitter had been definitely simplified, and consisted of a simple contact spring, as shown in Fig. 6 at D. The dots and dashes were obtained, as they are now, by a short and a long contact; this transmitter was soon modified as shown in Fig. 7.

Although the apparatus which we have just described operated satisfactorily in 1844, and was capable of transmitting news from the Washington Congress to Baltimore, it had not as yet reached the desired degree of perfection for being worked on a large scale. This is well shown by an interesting letter written by Prof. Page on the 26th of March, 1860, from which we give a few extracts:

"The life, the soul, of the electric telegraph is that wonderful little instrument called the receiving electro-magnet, invented by Prof. Wheatstone, of England. Prof. Morse also made use of a receiving electro-magnet, it is true, but one of such a construction that it was not applicable to a long circuit. The electro-magnet that he used upon the first telegraph line tried at the expense of the government between Washington and Baltimore was formed of a very strong curved iron bar whose magnetizing bobbins were made with strong No. 16 or 17 copper wire covered without care with common cotton. This electro-magnet, with its bobbins, was inclosed in a large box, which required the strength of two men to lift it. The wire of the bobbins was, moreover, of the same size as that of the line circuit. . . . The first trials of a wire of smaller diameter than the line wire for winding the electro-magnets of the Morse telegraph were made by me while Prof. Morse was in Europe. I had, in fact, substituted for the cumbersome electro-magnet of the apparatus a small receiving electro-magnet that occupied, as regards space, about half a cubic foot. I had at that time no knowledge of the experiments that had been undertaken by Prof. Wheatstone in Europe. It was not till his return to America that Prof. Morse, upon introducing into his apparatus a small Wheatstone electro-magnet that he had brought with him, obtained from his system results that were truly advantageous, and it is only from this time that dates his success." It was, then, through a series of successive modifications that the Morse apparatus finally became a practical one; but, whatever be the sources whence the inventor obtained the idea of these improvements, and however exaggerated be his pretensions regarding the date of his invention, we cannot refuse him the merit of having finally produced an apparatus that has contributed to a large extent to the development of telegraphy.—*A. Guerout, in La Lumiere Electrique.*

THE DRAWBAUGH TELEPHONES.

THE claims of Daniel Drawbaugh, of Eberly's Mills, Pa., to the priority of invention of the telephone are upheld by the The People's Telephone Company, who have adduced evidence of an interesting and, in a certain way, extraordinary, nature, in their defense, in the suit for infringement brought against them by the American Bell Telephone Company, and still pending in the New York Circuit Court. Mr. Drawbaugh places the date of his first conception of the electrical transmission of articulate speech as far back as 1860. He claims that electric telephony had been an accomplished fact with him for a dozen years when will,

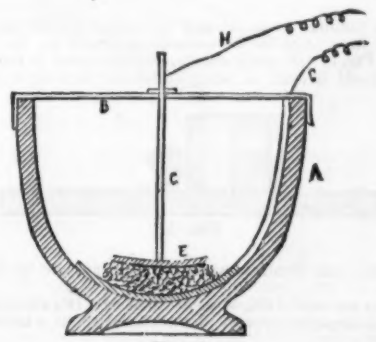


FIG. 1.

Edison, Gray, and the other claimants came upon the scene. The telephonic apparatus said to have been used by him has been produced in court, and witnesses to the number of one hundred and forty-five appeared, who identified and picked out the instrument or instruments which they had seen and heard speak of in Drawbaugh's shop, between 1867 and 1876. In this there is nothing extraordinary; but what is exceedingly strange, however, is the fact that these forms of apparatus all bear a more or less striking resemblance to the practical forms of telephonic apparatus that have been used by Bell and others, and introduced in common use. It would appear, in fact, from the study of these instruments that not only had Mr. Drawbaugh invented the art of telephony before 1876, but that he had exhausted every one of the resources of invention which have been drawn from by subsequent inventors in their aim to improve the art. For ourselves, we admit that these wonderful coincidences have taken our credulity rather by surprise, and left us rather skeptical in presence of this vast array of testimony. We have therefore preferred to let others speak first, whose disinterestedness cannot be questioned, and we have purposely deferred giving a description of these instruments until the present time, when we can express our own views by quoting the comments of some of our European contemporaries.

The earliest form of Drawbaugh transmitter, said to have been used in 1866, is represented in Fig. 1. A flexible cover, B, of membranous material, is stretched over the top of a china tea-cup, D. In the bottom of this cup is a mass of powdered material, F, resting on a metallic plate, to which one electrode is attached. A metal rod, C, extends down from the diaphragm, B, to a plate, E, that rests upon the powdered mass, for which Drawbaugh is said to have used many substances—bronze powder, finely divided metal, fine earth, etc.—though he ultimately adopted plumbago, pulverized charcoal, and other forms of carbon. This transmitter is substantially like the Edison carbon-button transmitter, and resembles even more closely the modification made

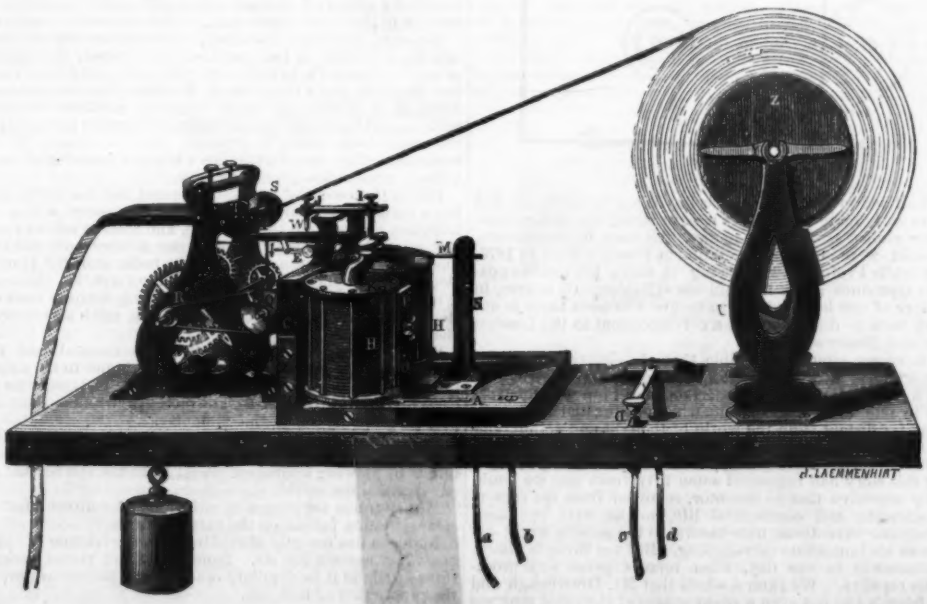


FIG. 6.—MORSE APPARATUS AS USED ON THE LINE BETWEEN WASHINGTON AND BALTIMORE.

by Signor Righi. The receiver (Fig. 2) used with this transmitter differs only in a few details from the first receivers used by Bell. A old mustard can, B, of tin, fastened on its side to a base board, A, is provided with a membrane, C, connected by a string, D, with an armature, F, supported

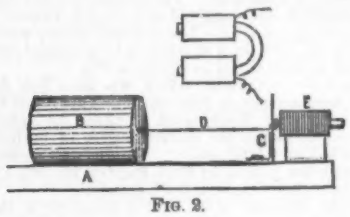


FIG. 2.

by a spring, C, so as to be in front of the two poles of an electro-magnet, E, of U form, made of a coil of insulated wire wrapped around a bent piece of iron. The connections were made as in Fig. 3.

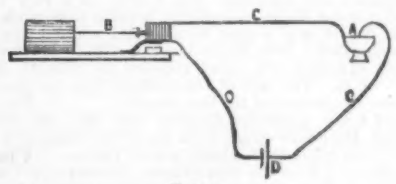


FIG. 3.

In the apparatus of 1867–1868, some improvements are noticeable. The transmitter (Fig. 4), made of a jelly tumbler, was provided with a wooden mouth-piece, B, which is of

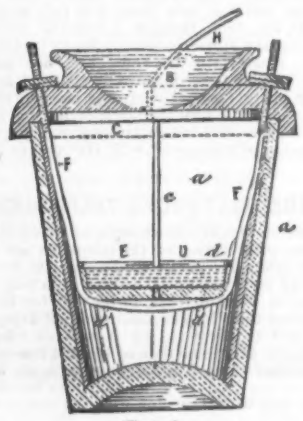


FIG. 4

a form familiar to us all, and the carbon powder was suspended in a tray by the connecting wire, *d d*. In the receiver (Fig. 5) the armature plate was fastened to the membrane itself instead of being connected to it by a string.

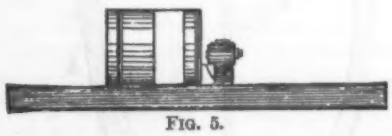


FIG. 5.

This form is identical with a receiver made by Bell in 1876.

It was not until 1870, it seems, that Mr. Drawbaugh produced a telephone capable of working without a battery by

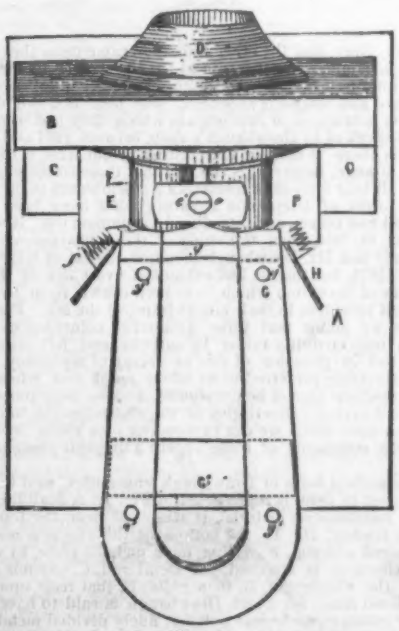


FIG. 6.

employing a permanent magnet. This instrument, shown in Fig. 6, is substantially like the "lecture model" used by Bell in 1877. Toward 1874 Mr. Drawbaugh would seem to have returned to the use of an electro-magnet excited by a battery. This apparatus is provided with means of adjusting the electro-magnet. This form (Fig. 7) is said to have been preserved complete, and to be still in excellent work-

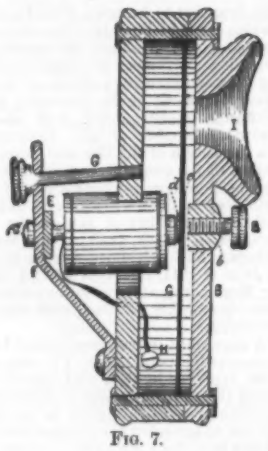


FIG. 7.

ing order. In 1875 the receiver had become transposed into the form shown in Fig. 8, which is almost identical in all details with a form of receiver commonly used here and abroad. The magnet, D, is made of spiral form, and the

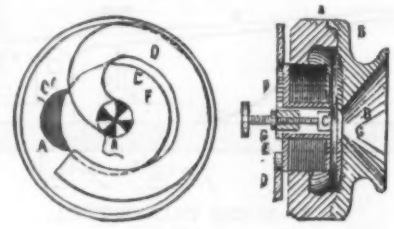


FIG. 8.

core, C, inside the wire coil, F, is made adjustable by means of a screw. In 1876 appeared a very perfect and compact form of carbon transmitter (Fig. 9.).

A plate diaphragm was used, and instead of loose carbon

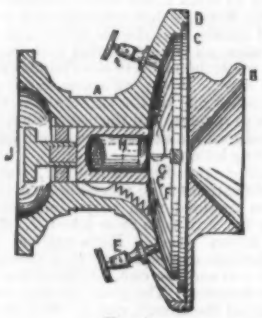


FIG. 9.

powder, blocks of hard retort carbon were employed. Finally, during the same year, Mr. Drawbaugh had given a practical commercial form to his apparatus. He had placed his transmitter in a box (Fig. 10) suitable to be fastened to

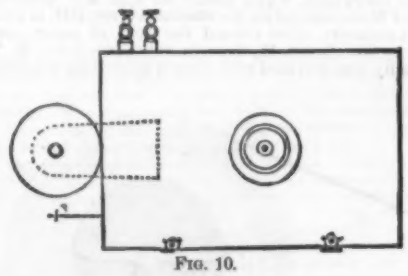


FIG. 10.

the wall and provided with a bell. What is more, he had put in an induction oil. This was, in short, the Blake transmitter almost as we find it to-day. All these improvements, it is said, were in perfect operation at Eberly's Mills in 1876, even while Prof. Bell was trying to make his crude apparatus reproduce a few incompleted syllables. Of course, in the face of one hundred and forty-five witnesses there is not much room to doubt, but, as a correspondent to the London *Electrical Review* says:

"It seems almost incredible that the electric telephone, 'the greatest marvel hitherto achieved by the electric telegraph,' should have been conceived and brought to perfection by one man, and that man an uneducated workman, living far distant from the world of thought and action, and the whole accomplished within a period of ten years."

Our French contemporary, *La Lumière Electrique*, says: "If this story had happened some fifty years ago we could easily conceive that an inventor, removed from the centers of scientific and commercial life, might have produced wonderful inventions unbeknown to the outside world except to his immediate surrounding. But the thing is almost inadmissible in our day, when reports spread with prodigious rapidity. We cannot admit that Mr. Drawbaugh and his friends had not even a slight notion of the rapid progress of telephony."

An Italian electrical journal, *Il Giorno*, says: "In fact, the resemblance of the Drawbaugh apparatus with that now in use gives rise to suspicions as to the date of their invention—suspicions which the silence held concerning these instruments certainly does not dispel; and it seems to us that the extraordinary claims of Mr. Drawbaugh need more ample confirmation."—*Electrical World.*

THE SOURCE OF ATMOSPHERIC ELECTRICITY.*

WE have seen that, taking for granted the electrification of clouds, all the ordinary phenomena of a thunderstorm (except *globe* lightning) admit of easy and direct explanation by the known laws of statical electricity. Thus far we are on comparatively sure ground.

But the case is very different when we attempt to look a little farther into the matter, and to seek the source of atmospheric electricity. One cause of the difficulty is easily seen. It is the scale on which meteorological phenomena usually occur; so enormously greater than that of any possible laboratory arrangement that effects, which may pass wholly unnoticed by the most acute experimenter, may in nature rise to paramount importance. I shall content myself with one simple but striking instance.

Few people think of the immense transformations of energy which accompany an ordinary shower. But a very easy calculation leads us to startling results. To raise a single pound of water, in the form of vapor, from the sea or from moist ground, requires an amount of work equal to that of a horse for about half an hour! This is given out again, in the form of heat, by the vapor when it condenses; and the pound of water, falling as rain, would cover a square foot of ground to the depth of rather less than one-fifth of an inch. Thus a fifth of an inch of rain represents a horse-power for half an hour on every square foot; or, on a square mile, about a million horse power for fourteen hours! A million horses would barely have standing room on a square mile. Considerations like this show that we can account for the most violent hurricanes by the energy set free by the mere condensation of vapor required for the concomitant rain.

Now the modern kinetic theory of gases shows that the particles of water-vapor are so small that there are somewhere about three hundred millions of millions of millions of them in a single cubic inch of saturated steam at ordinary atmospheric pressure. This corresponds to $\frac{1}{1000}$ or so of a cubic inch of water, *i. e.*, to about an average raindrop. But if each of the vapor particles had been by any cause electrified to one and the same potential, and all could be made to unite, the potential of the raindrop formed from them would be fifty million million times greater.

Thus it appears that if there be any cause which would give each particle of vapor an electric potential, even if that potential were far smaller than any that can be indicated by our most delicate electrometers, the aggregation of these particles into raindrops would easily explain the charge of the most formidable thundercloud. Many years ago it occurred to me that the mere *contact* of the particles of vapor with those of air as they interdiffuse according to the kinetic theory of gases, would suffice to produce the excessively small potential requisite. Thus the source of atmospheric electricity would be the same as that of Volta's electrification of dry metals by contact. My experiments were all made on a small scale, with ordinary laboratory apparatus. Their general object was, by various processes, to precipitate vapor from damp air, and to study either (1) the electrification produced in the body on which the vapor was precipitated; or (2) to find on which of two parallel, polished plates, oppositely electrified and artificially cooled, the more rapid deposition of moisture would take place. After many trials, some resultless, others of a more promising character, I saw that experiments on a comparatively large scale would be absolutely necessary in order that a definite answer might be obtained. I communicated my views to the Royal Society of Edinburgh in 1875, in order that some one with the requisite facilities might be induced to take up the inquiry, but I am not aware that this has been done.

I may briefly mention some of the more prominent attempts which have been made to solve this curious and important problem. Some of them are ludicrous enough, but their diversity well illustrates the nature and amount of the difficulty.

The oldest notion seems to have been that the source of atmospheric electricity is aerial friction. Unfortunately for this theory, it is *not* usually in windy weather that the greatest development of electricity takes place.

In the earlier years of this century Pouillet claimed to have established by experiment that in all cases of combustion or oxidation, in the growth of plants, and in evaporation of *salt* water, electricity was invariably developed. But more recent experiments have thrown doubt on the first two conclusions, and have shown that the third is true only when the salt water is boiling, and that the electricity then produced is due to friction, not to evaporation. Thus Faraday traced the action of Armstrong's hydro-electric machine to friction of the steam against the orifice by which it escaped.

Saussure and others attributed the production of atmospheric electricity to the condensation of vapor, the reverse of one of Pouillet's hypotheses. This, however, is a much less plausible guess than that of Pouillet; for we could understand a particle of vapor carrying positive electricity with it, and leaving an equal charge of negative electricity in the water from which it escaped. But to account for the separation of the two electricities when two particles of vapor unite is a much less promising task.

Peltier (followed by Lamont) assumed that the earth itself has a permanent charge of negative electricity whose distribution varies from time to time, and from place to place. Air, according to this hypothesis, can neither hold nor conduct electricity, but a cloud can do both; and the cloud is electrified by conduction if it touch the earth, by induction if it do not. But here the difficulty is only thrown back one step. How are we to account for the earth's permanent charge?

Sir W. Thomson starts from the experimental fact that the layer of air near the ground is often found to be strongly electrified, and accounts for atmospheric electricity by the carrying up of this layer by convection currents. But this process also only shifts the difficulty.

A wild theory has in recent times been proposed by Becquerel. Corpuscles of some kind, electrified by the outbursts of glowing hydrogen, travel from the sun to the upper strata of the earth's atmosphere.

Mabry traces the source of electricity to a direct effect of solar radiation falling on the earth's surface.

Luddons has recently attributed it to the friction of aqueous vapor against dry air. Some still more recent assumptions attribute it to capillary surface-tension of water, to the production of hail, etc.

* By Prof. Tait. Read at the meeting of the Scottish Meteorological Society on March 17, and communicated by the Society.

Blake, Kalischer, etc., have lately endeavored to show by experiment that it is not due to evaporation, or to condensation of water. Their experiments, however, have all been made on too small a scale to insure certain results. What I have just said about the extraordinary number of vapor particles in a single raindrop shows that the whole charge in a few cubic feet of moist air may altogether escape detection.

And so the matter will probably stand, until means are found of making these delicate experiments in the only way in which success is likely to be obtained, viz., on a scale far larger than is at the command of any ordinary private purse. It is a question of real importance, not only for pure science but for the people, and ought to be thoroughly sifted by means which only a wealthy nation can provide.

PROF. HEYER ON COMETS.

To the Editor of the Scientific American:

In your SUPPLEMENT No. 433 there is an article (from the pen of Prof. Hughes) which sets forth exactly the same theory that I advanced in 1862 in New Orleans, and have since illustrated before my classes in Homer College and elsewhere. Although it is not probable that the writer ever heard of my theory, yet as it is one which must eventually be universally accepted, I should like to be credited with priority of discovery.

In 1881, I wrote an account of my views, which I mailed to you, and received an answer by letter restating the usually accepted incandescent theory, and in October, 1882, I wrote again on the same subject.

As a short statement of the theory may be interesting to your readers, I inclose an extract from my letter to Prof. J. Norman Lockyer, dated March 6, 1883, in relation to theories advanced by him:

1. If the tail of the comet indicates the waste of the head, we should look for the tail in the path which the comet has passed over. Instead of this, we find the tail at an angle (more or less great) with the orbit. The comparison of the tail to the "smoke of a locomotive at high speed" is hardly admissible, as it is the resistance of the air which causes this appearance. Were there no atmosphere, the smoke would accompany the engine, no matter how great its speed.

2. According to the laws of gravitation, the tail, if *composed of matter*, would surround the comet as the atmosphere does our earth, instead of following after it.

We have no evidence of the atmospheres of the planets trailing behind them, nor would it be possible for matter to stay away from its center of attraction.

3. Attraction does not act along a line, but from a center. We cannot imagine a mass of gaseous or other matter, many millions of miles long, forming itself into a conical shape in obedience to any law of attraction with which we are acquainted. Even supposing that this could be the case, the tail would then fly toward the sun in advance of the comet, instead of following after.

4. An objection to the incandescent theory of the tail is that it would manifestly be impossible for a particle of matter to retain its heat at the temperature of incandescence for so long a time, and at so great a distance from its source, the head of the comet.

5. Were the particles of matter, said to compose the tail, continually emanating from the head, the ultimate destruction of the comet would be a matter of a few hours, instead of thousands of years.

No matter how ethereal the substance, the loss of several billions of cubic miles of matter per day could not go on without exhausting its source.

6. The great velocity with which the comet passes around the sun necessitates an enormously greater velocity of the tail, especially of those parts near the end. If we reflect upon the circumstance of an immense column of vapor, or of solid particles, switching around the sun at such an incredible speed and still preserving its integrity as a column, we shall see the absurdity of the idea.

7. The light of stars seen through the comet's tail is not refracted, as it would be were the tail composed of gaseous matter or of minute solid particles.

8. Were the tail an electrical emanation from the head, as some maintain, it would manifestly extend between the comet and the sun, instead of away from both.

Electricity must act *between* two points—there must be a connection, a circuit, before we have electrical results.

There is certainly no fact in electrical science better established than this.

I think that these considerations are sufficient to show that the comet's tail is neither matter nor electricity.

Neither is it demonstrable that the comet itself (*i. e.*, the head) is self-luminous.

As an offset to the popular theories, I offer my own as affording a reasonable explanation of all the phenomena presented by these bodies. I have held this theory for over twenty years, and have frequently explained it in lectures.

According to this theory, the comet's head is composed of a nearly transparent mass of nebulous or gaseous matter, considerably condensed at the center, forming the nucleus. This is illuminated by the sun, and rendered visible to us. This mass of matter also acts as a lens, and the sun's rays passing through form a long cone of light similar to that produced by the sunlight shining through the lens of a solar camera.*

In the former case, the cone of light from the comet's head may be seen in the same manner reflected from the particles of ether.

The *nucleus*, being denser than the other parts, casts a shadow, which we see in the dark streak through the middle of the tail. It may be objected that we could not see this cone of light sidewise, as it would be of no greater intensity than the sunlight continually streaming through space, but if the objector will place a lens in the sunlight, and shake a little dust or smoke in the light, the cone of transmitted light will be distinctly seen, notwithstanding the presence of the other rays which are not refracted.

If we suppose the rear portion of the comet to be irregular in shape, having facets or irregularities, these facets will refract the sunlight at different angles, and we will have a comet with more than one tail. The disturbances going on in the body of the comet itself may also modify the appearance of the tail. The increased brightness on approaching the sun is what we would naturally expect.

In the case of comets whose tails have disappeared on their approach to the sun, they probably were seen by us endwise, or else the action of the sun upon the matter of the comet rendered it opaque, so that it would no longer transmit light.

The *curvature* of the tail may readily be accounted for by the fact that the end of the tail being farther from us than the head, the light takes longer to reach us, and meanwhile the head has moved forward some distance, so that we see the old position of the tail and the new position of the head at the same moment; consequently the tail appears curved. Our position with respect to the comet also modifies the length and curvature of the tail.

W. D. HEYER.

Elizabeth, April 25, 1884.

* It is necessary to stand at some distance to see the cone illuminated.

HOW TO MAKE AN INCUBATOR

EXPERIMENTS with the incubator here given have been made all over the country. It is one that is in actual use, and has always given satisfaction. Having secured permission from the originator, we here illustrate it for the benefit of our readers.

To make this incubator, get your tinner to make you a tank fifteen inches wide, thirty inches long, and twelve inches deep, of galvanized iron or zinc, the iron being preferable. On the top should be a tube one inch in diameter and eight inches high. In front should be another tube, nine inches long, to which should be attached a spigot, as illustrated in 2. Having made your tank, have what is called the ventilator made, which is a box with a bottom but no top. The ventilator should be eight inches deep, and

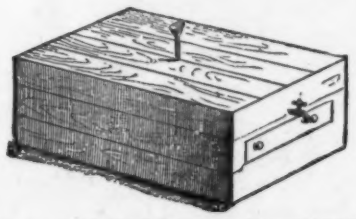


FIG. 1.—THE HOT WATER INCUBATOR.

one inch smaller all round than the tank, as the tank must rest on inch boards. In the ventilator should be four or six tin tubes, one-half inch in diameter, and six inches long. They should extend through the bottom, so as to admit air from below, and to within two inches of the top, or a little less. Now make an egg drawer, which is a frame of wood, three inches deep, having no top or bottom, except that the front should be boxed off and filled with sawdust, which is covered over afterward with a piece of muslin, to keep the sawdust from spilling. This box in front of the drawer exactly fits the opening in Fig. 3, when the egg drawer is in its place. Of course the egg drawer must be made longer than the tank and ventilator, in order to allow for this space which it fills in the opening, which is the packing all around the incubator. The bottom of the egg drawer should be made by nailing a few slats lengthwise to the under side, or rather, fitting them in nicely, and over the slats in the inside of the drawer a piece of thick, strong muslin should be tightly drawn. On this muslin the eggs are placed, in the some position as if laid in a hen's nest, and it allows the air to pass through to the eggs for ventilation. The eggs can be turned by hand, marked for designation, or an egg

FIG. 2.—THE TANK.

Observe that the tubes on the top and the spigot are quite long, in order that they may extend through the packing of sawdust which is to surround it. This tank is to have a close-fitting covering (top and sides) of wood, to resist pressure of water. The bottom is not to be covered.

turner may be made by fastening slats crosswise to one on each side running lengthwise, something like a window lattice, and when the eggs are placed between these slots, by merely pushing the frame the eggs will turn over, exactly on the same principle that an egg will roll when it is pushed by a block, a book, or anything else; but we believe the method is patented, and do not advise infringement.

Having prepared the tank, let it be covered with a box, but the box must not have any bottom. This is to protect the tank against pressure of water on the sides, and to assist in retaining heat. Such being done, place your ventilator first, egg drawer next, and tank last. Now place a support under the tank and the box, or have them rest on rods, and as the weight of water will be great in the center, the iron

FIG. 3.—DRAWER OPENING.

Shows the thick packing, which is noticed at the opening. This extends all around. The front of the egg drawer (Fig. 4) fits in its place in order to complete the surrounding packing, when the incubator is closed, as at Fig. 1.

rods should be placed crosswise under the tank every six inches. Now fasten the three apartments (ventilator, egg drawer, and tank) together, with boards nailed to the sides and back and front (of course leaving the opening for the egg drawer), care being taken to drive no nails in the egg drawer, as it must move in and out, and should have a strong strip to rest on for that purpose. Having completed these preparations, make a larger box to go over all three, so that there will be a space on the sides, back, front, and on top; but as the ventilator must be filled with sawdust to within one inch of the top of the tubes, it serves for the bottom packing. Make the outer box so that there will be room for filling all around the inside box with sawdust, and also on top, being careful to let the tube for pouring in the water come through, as also the spigot in front. About four inches or so thickness of sawdust is sufficient, according to preference. The front of the incubator must be packed also, but an idea of how it should be done may be learned by observing the opening in Fig. 3, which is so constructed that the box in front of the egg drawer, 4, exactly fits into it, and completes the packing when the drawer is shut. The incubator should be raised from the floor about an inch, when completed, to allow the air to pass under and thence into the ventilator tubes.

The incubator being complete, the tank is filled with boiling water. It must remain untouched for twenty-four hours, as it requires time during which to heat completely through. As it will heat slowly, it will also cool slowly. Let it cool down to 120°, and then put in the eggs, or, what is better, run it without eggs for a day or two in order to learn it, and notice its variation. When the eggs are put in, the drawer will cool down some. All that is required then is to add about a bucket or so of water once or twice a day, but be careful about endeavoring to get up heat suddenly, as the heat does not rise for five hours after the additional bucket of water is added. The tank radiates the heat down on the eggs, there being nothing between the iron bottom of the tank and the eggs, for the wood over and around the

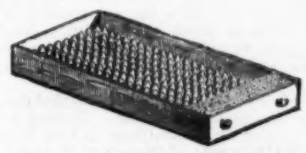


FIG. 4.—THE EGG DRAWER.

The space just in front of the egg is the portion partitioned off to fit in the opening at Fig. 3. The egg drawer is therefore *longer* than the tank and ventilator.

tank does not extend across the *bottom* of the tank. The cool air comes from below in the ventilator pipes, passing through the muslin bottom of the egg drawer, to the eggs. The 15x30 inch tank incubator holds 100 eggs if turned by hand, but less if the eggs are placed between slats. Lay the eggs in, the same as in a nest, promiscuously.

DIRECTIONS.

Keep the heat inside the egg drawer as near 103° as possible; the third week at 104°. Avoid opening the egg drawer frequently, as it allows too much escape of heat. *Be sure your thermometer records correctly*, as half the failures are due to incorrect thermometers, and not one in twenty is correct. Place the bulb of the thermometer even with the top of the eggs, that is, when the thermometer is lying down in the drawer. The upper end should be slightly raised, so as to allow the mercury to rise, but the bulb and eggs should be of the same heat, as the figures record the heat in the bulb and not in the tube. Keep a pie pan filled with water in

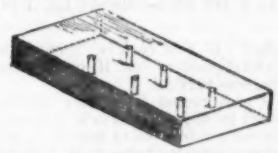


FIG. 5.—THE VENTILATOR.

The tubes admit air from below, which passes into the egg drawer above through the muslin bottom of the egg drawer, to the eggs. The eggs rest upon the muslin, which is tightly drawn over narrow slats running lengthwise the bottom of the drawer.

the ventilator for moisture, and keep two or three moist sponges in the egg drawer, displacing a few eggs for the purpose. Turn the eggs half way round twice a day at regular intervals. Let the eggs cool down for fifteen minutes once every day, but do not let them cool lower than seventy degrees. No sprinkling is required if the sponges are kept moist. If the heat gets up to 110°, or as low as 60° for a little while, it is not necessarily fatal. Too much heat is more prevalent than too little. A week's practice in operating the incubator will surprise one how simple the work is. The tank will be troublesome to fill at first, but the matter will be easy after it is done, as it can be kept hot. Heat the water in two or more boilers, as a large quantity will be required, and pour it in through the tube on top of the incubator boiling hot, using a funnel in the tube for the purpose. Just at the time of hatching out do not be tempted to frequently open the drawer. Cold draughts are fatal. Patience must be exercised.

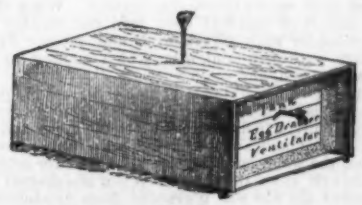


FIG. 6.

Here we remove the front of the incubator in order to show the positions of the ventilator, egg drawer, and tank. First the ventilator, then the egg drawer (which of course should be longer than the others in order to fit in the opening shown at Fig. 3, but which we did not do here in order to mark the places), and on the top is the tank. When the front is completed, the incubator is seen at Fig. 1.

BROODERS.

An excellent brooder may be made with a tank of hot water, covered well, the same as the incubator. A piece of

muslin or woolen cloth should be next the bottom to prevent burning the chicks Keep the heat among the chicks at about ninety degrees. Let the tank rest on inch boards, with no opening under it, but, in front, a fringe should hang in front for the chicks to run in and out under the tank, A little yard may be constructed of glass sash, something like a hot-bed. Feed the chicks four or five times a day, at first on hard boiled eggs, chopped fine, giving them also a little milk, fine screenings, and millet seed. After they are a week old feed anything they can eat, but never feed meal in a raw state, as it should be scalded first. Keep a little sand, fine gravel, and bone meal within reach of them all the time, and see that they are always dry, clean, and warm. Do not crowd them, as the fewer the number together the better, and never have different ages together.

This incubator will hatch chicks, ducks, turkeys, or guineas, and we see no reason why it should not hatch the egg of the ostrich or anything else as well. *Chicks batched by incubators, if rightly cared for, do better than with hens, and are stronger and more vigorous.

We have endeavored here to embody all the answers to questions that we suppose may be asked. Should you be in doubt, read the directions carefully again. We trust that with the help of our illustrations our readers will have no difficulties in the way.—*The Poultry Keeper.*

SUBMARINE EXPLORATIONS.*

ASIDE from the sounding and dredging apparatus on board the Talisman there were special instruments that served for collecting water at different depths. It was, in fact, very important to know the composition of the ocean water at the depth at which a given fauna lived, in what proportions gases were dissolved therein (sometimes at profound depths), and finally what degree of saliniess it possessed.

This kind of research had already occupied the naturalists on board the Challenger and Blake. During the course of the cruise made by the Travailleur in 1883 in the Gulf of Gascony, along the coasts of Spain and Portugal, and in the Mediterranean, water was brought up from the greatest depths by means of apparatus constructed according to plans devised by Mr. E. Richard, assistant to Mr. Villegente.

The result of these first trials was excellent, and Mr. Bouquet de la Grye, to whom had been intrusted the examination of the specimens obtained, gave a *resumé* of his researches in a memoir presented to the *Academie des Sciences*, and different and very important facts relative to the physics of the globe are therein called attention to by this learned hydrographic engineer.

On board the Talisman the water bottles used were those that had been employed during the cruise of the Travailleur. Each of these apparatus consists of a very thick metallic tube terminating at either end in a truncated cone. Under the upper of these latter and over the lower one there is a cock that closes by means of a long lever projecting out of the apparatus like the one in the reversible thermometer

FIG. 2.—EFFECT OF SEA-PRESSURE UPON CORK.

already described. If this cock be opened the lever will assume a horizontal position, but if, on the contrary, it be closed it will become vertical.

This cock is constructed in a very peculiar manner. When it is opened its key rests upon an internal central rod to which is adapted a rubber valve that closes an aperture in an internal partition situated beneath it. The valve is then lifted, and the water can enter the cylinder freely. If, on the contrary, the valve is closed, the extremity of the internal central rod fits into a cavity in the key. The valve, under the action of a spring, falls and hermetically closes the aperture in the internal partition.

The apparatus was used as follows: The extremity of a hemp sounding line, arranged in several coils and supported upon large horizontal wooden rods, was loaded with a very heavy weight. The line thus placed was wound up and passed a few times around the large drum of the lifting windlass, and then, by means of pulleys arranged in a peculiar way, it was raised obliquely above and outside of the vessel.

To the extremity that was thus hanging over the water, there was attached a weight that varied according to the depth that was to be reached. After this a bottle with open cocks was fixed along the line. The lever of these cocks had consequently a horizontal position. Above the water bottle there was attached a reversible thermometer, in order that the temperature of the water that was to be collected might be accurately ascertained.

These arrangements made, the apparatus were lowered into the sea, and five hundred meters of line were paid out. As the velocity of descent kept gradually increasing, and might have become too great, the windlass brake was made to act and thus render the motion regular. When the five hundred meters of line had been paid out, another bottle and thermometer were affixed to it and another five hundred meters were unwound, after which another set of apparatus was affixed, and so on.

The number of apparatus thus placed depended upon the depth that existed at the spot where observations were being made. During their entire descent the lower and upper cocks were open, and a current was set up in the interior of the bottles, the water entering through the lower orifice and making its exit from the upper. When the desired depths were reached the line was prevented from unwinding further and the apparatus were allowed to remain for some little time in place.

Then there was allowed to fall from the boat a heavy cast iron ring, through the center of which passed the line that supported the bottles and thermometers. The thickness of the ring and the diameter of its opening were so calculated as to permit it to pass over each bottle and thermometer, and depress their levers without any danger of getting fastened to them. The motion of the levers brought about in the first place an inverting of the thermometers, and in the second a closing of the cocks.

At the moment the latter closed, the interior, central rods, both lower and upper, became disengaged, and the valves applied themselves against the internal orifices of the bottles. The double closing thus effected, first by the cock, and then by the valve, was designed to absolutely prevent the introduction of the liquid that they were to traverse upon being drawn up, as well as an expansion of the gases contained in the water that they held.

These gases, as a consequence of the rapid decomposition that they underwent, had, in fact, a very strong tendency to escape. But the expansion that thus occurred in the interior of the bottles had for effect (as pointed out by Mr. Milne Edwards in his report upon the cruise of the Travailleur) "to press more strongly against the rubber valves and to close the apertures more perfectly."

When water was thus taken from great depths, care was always had to collect a specimen from the surface and to note its temperature, so as to obtain a point of comparison.

At every sounding there was thus brought on board a specimen of water from the bottom touched, and this was easily preserved by at once sealing the tube that contained it.

Dredgings at great depths require considerable time for their performance, so it often happens that the net that has been sent to the bottom can be brought on board again only at a late hour in the day. In the tropics, where most of the cruising of the Talisman was done, night came on early, for twilight in these regions is of but very short duration. To offset this disadvantage precaution was taken during the fitting up of the Talisman to put in electric apparatus capable of yielding a sufficiently bright light to make it possible, when the trawl was brought on board at night, to search

FIG. 1.—EFFECT UPON A FISH (NEOSCOPELUS MACROLEPIDOTUS) OF THE EXPANSION OF ITS SWIMMING BLADDER UPON REACHING THE SURFACE.

The collecting of specimens of water from great depths was, as may be gathered from the preceding *expose*, an operation that took quite a good deal of time. So on board the Talisman an endeavor was made to simplify the maneuvers when it was desired to obtain water, not for a study of the gases that it contained, but simply to look for any germs that it might hold in suspension.

The following was the process employed: Thick glass tubes, drawn out at their extremities, and closed by means of a glass-blower's lamp after a vacuum had been created in them, were attached to the metallic tube that inclosed the thermometer. They were fixed in such a way that when the latter turned over, one of their tapering points struck the lower part of the thermometer frame and broke off. Thereupon the water rushed into the tube, from which it was unable to escape owing to the small diameter of the inlet orifice.

out with the greatest care all the objects collected, however small they might be.

With this end in view, a Gramme machine that Col. Perrier had had the kindness to loan was set up on the bridge. This was actuated by a second Brotherhood motor placed alongside of the Thibaudier sounding apparatus, and was put in communication with a series of Edison lamps that lighted either the net or the interior of the laboratory. At the same time it communicated with a regulator that permitted of projecting a bright light upon the sea. The coming of the net to the surface was thus very easily watched.

Lt. Jacquet had in charge the management and surveillance of these apparatus, and we are happy to state just here that, thanks to his devoted aid, our night researches were rendered very easy and extremely successful.

The Edison lamps were not only used for lighting the

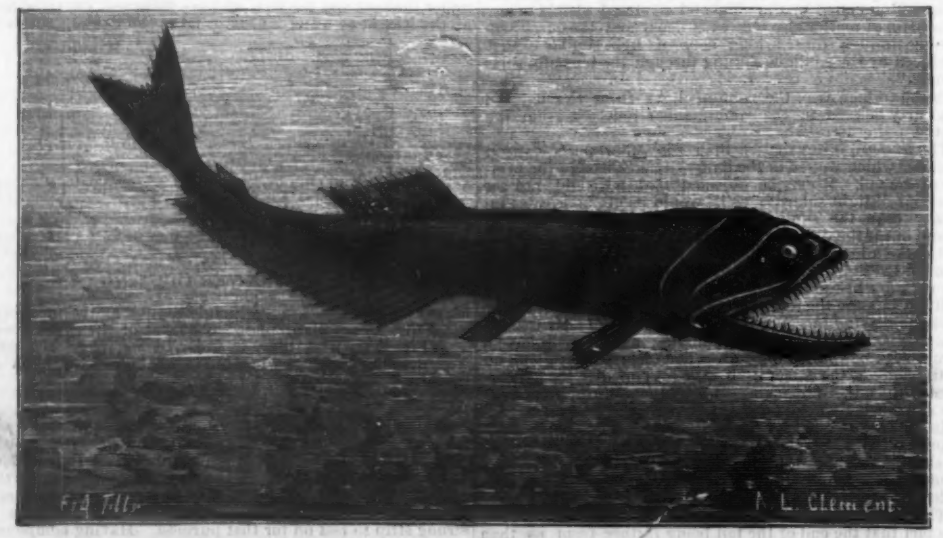


FIG. 3.—NEOSTOMA BATYPHILLUM, FROM A DEPTH OF 2,220 METERS. (Slightly reduced.)

* Continued from page 6907.

vessel, but were also at different times let down into the sea in order to attract fishes into nets that had been previously set. No idea can be formed of the beauty of the spectacle that was to be seen when these brilliant, luminous foci were let down into the water. The sea all around them lighted up with the most brilliant and most changeable tints, that made it seem as if we had before our eyes some of those beautiful medusæ which, like luminous disks, rise, descend, roll, and disappear amid the waves in order to exhibit themselves a few instants afterward more brilliant than ever.

Now that we have succinctly made known the means for research that the committee upon the Talisman had at its disposal, it remains for us to speak of the value of the harvest that it gathered. In the first place, we must say that the latter much exceeded all the hopes that had been conceived, and visitors who attend the exhibition (at the Jardin des Plantes) of the collection that has been made within the short space of three months, will be surprised at the abundance and variety of the animal forms submitted to their inspection.

The state of preservation of the animals is especially remarkable, the fishes alone seeming to be slightly spoiled. These latter animals, contrary to what might have been expected, could not withstand violent shocks, after entering the net. Their alteration is due simply to the phenomena of decomposition that they necessarily underwent. There always exists in fishes a very singular organ consisting of a closed bag situated over the intestines, against the vertebral column. The presence of this apparatus, called a swimming bladder, which by its mode of development seems to correspond to lungs, permits fishes to rise or descend in water with great facility. In a fish taken at quite a great depth and brought to the surface, the gases inclosed in this bladder continue to expand to a greater and greater volume. It results that the bladder finally, as a consequence of such expansion, exerts great pressure upon the abdominal wall, which, progressively ceding, gradually loses the scales with which it is covered. When the expansion of the bladder is pushed to its last limits, its fore extremity is observed to repel the stomach, penetrate the mouth, and finally project externally. The pressure that it then exerts upon the top of the buccal cavity is such that the latter gives way under the stress, and the eyes are finally expelled from their sockets. We have endeavored in Fig. 1, taken from a specimen at the exhibition, to show in what state fishes taken at great depths reach the surface.

The enormous pressure that is exerted upon the fishing apparatus when lowered to great depths may be appreciated by seeing the change that occurs in one of their constituent parts. In order to hold the neck of the trawl-net wide open there is arranged in the interior of the latter a series of large cork disks strung upon a cord. These disks before being used have quite a large diameter, but after a few days' use they exhibit no more than half their first dimensions. Under the influence of the great pressure that they have been obliged to undergo the tissues of which they are composed become greatly compacted, and they take on at the same time the consistency of a piece of wood. In Fig. 2 we reproduce, side by side, two such cork disks, one that has been employed and one that has not. The reductions are made to the same scale.

During the cruise of the Travailleur, the submarine exploring apparatus left much to be desired, and the capture of fishes was of so rare occurrence that Mr. Milne Edwards says in his reports that the taking of one of these animals "was considered as a genuine event." It was no longer the same this year during the cruise of the Talisman, thanks to the use of the trawl-net. Nearly all the dredgings were followed by the capture of a few fishes, and sometimes the number of individuals brought aboard was surprising. Thus on the 29th of July, in lat. 16° 52' and long. 27° 30', there were taken in a single haul of the net 1,031 fishes. The depth was 450 meters.

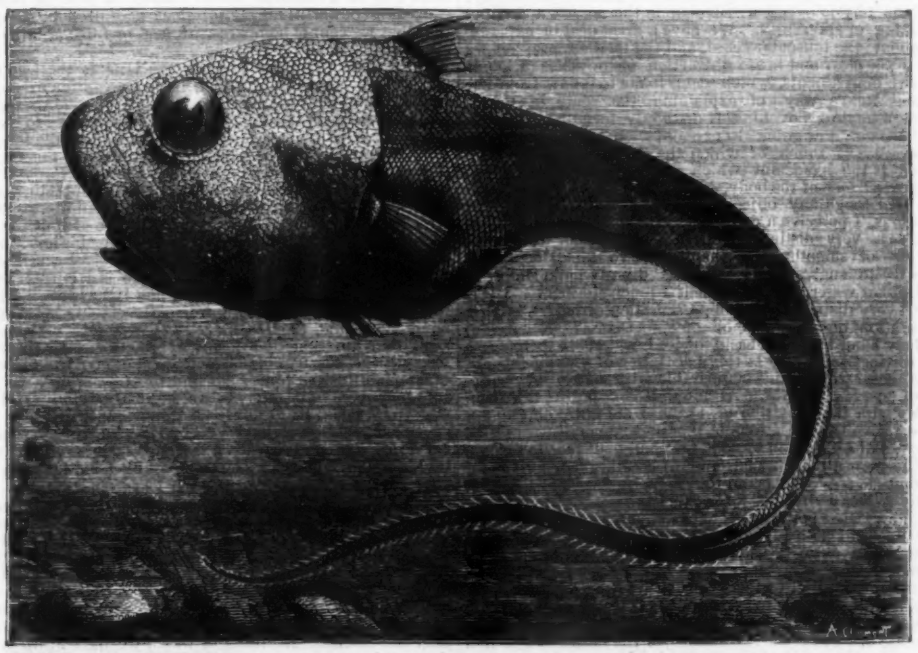


FIG. 4.—MACRURUS GLOBICEPS, FROM A DEPTH OF BETWEEN 1,400 AND 5,000 METERS. (½ Natural Size.)

The most interesting surface fishes to mention are the large sharks and a small species peculiar to the Sargasso Sea —the *Antennarius marmoratus* (Bl. Sch.).

Sharks (*Charcarius glaucus*) were especially noticed between Senegal and the Cape de Verd islands. They followed our vessel in numerous bands, and we often saw them accompanied by their "pilots," that is to say, fishes known to the ancients as *Pompilus*, and designated by our present naturalists as *Naucrates ductor*. It seems that the *Naucrates* serve as guides to the sharks, and that the latter, out of gratitude for the services that they render, never hunt them. It is certain that the *Naucrates* that we saw were living in perfect accord with the sharks. They swam around them and sometimes placed themselves against their bodies, beneath the pectoral fin. These fishes, whose form closely recalls that of the mackerel, were of a grayish blue, darker toward the back than upon the belly. Their body was surrounded with wide vertical bands of a beautiful blue. The pectoral fins were whitish, and the ventral were black, while the tail had a blue tint. We found the same species of shark in the Sargasso Sea.

FIG. 5.—EUSTOMIAS OBSCURUS, FROM A DEPTH OF 2,700 METERS. (Natural Size.)

It was amid the floating vegetation formed of sargasso that the second species of surface fish just mentioned was seen. The *Antennarius marmoratus* is one of the strangest animals ever observed. Its back is provided with long appendages, and its fins, which are elongated, enlarged at their extremities, and digitate, serve it after a fashion as paws by means of which it circulates among the algæ that shelter it. At the time of reproduction it constructs a nest by forming, through tough, slimy filaments, balls of sargasso upon which it deposits its eggs. These balls float, buffeted by the waves, and when the young are hatched they probably find in the interior of them a secure asylum.

The *Antennarius*, like all animals living in the midst of the Sargasso, such as crustaceans, mollusks, etc., possesses the same tints as this alga. The color of their bodies —marbled with brown, yellow, and white—entirely harmonizes with that of the environment by which they are surrounded, and it is only by paying minute attention in researches that they can be detected. It is evident that this similitude between the color of the body and the color of the environment results in allowing the animals to more easily hide themselves and thus succeed in escaping the searches of their enemies. But, as Mr. Milne Edwards has observed, although the livery of the sargasso can be considered as a protection to the animals that wear it, it becomes in certain cases a danger for them, since, owing to it, the carnivorous species that have assumed it can very easily approach their prey without being perceived.

The bottom fishes taken by the Talisman are referable to a large number of genera and species. An examination of them gives us a series of general facts of the highest interest. The first question that we may ask ourselves in studying them is whether there exist genera and species of fish that are characteristic of given depths, that is to say, whether the fish fauna is different at successive depths of one, two, three, four, or five thousand meters? This question we can answer in the affirmative, since it appears from the dredgings made that certain species have a perfectly limited distribution. In order to reach this conclusion it required numerous researches, in consequence of the strange fact that certain species of fishes are found in the sea at depths of from 600 up to nearly 3,000 meters. Thus a fish presenting the same organic structure is capable of living under pressures varying from a half ton to one and two tons and even more. After this it may be asked how it is that characteristic forms exist at definite depths, since, in the presence of so large zones of distribution, it would seem that the fauna of the abyss ought to remain the same. The explanation of this so singular a fact, is that the fishes that we find at from 600 to nearly 3,600 meters depth do not continually inhabit the same localities. They exhibit themselves as travelers; they rise and descend successively in the abysses of the sea, and, when they perform these journeys, they move gently, so as to undergo slow expansions and compressions. We shall mention some of the species that have permitted us to learn about these so remarkable peregrinations. Thus we found the *Alepocephalus rostratus* at from 868 up to 3,650 meters, the *Scopelus maderensis* at from 1,090 to 3,655 meters, *Lepioderma macrops* at from 1,153 to 3,655, and *Macrurus affinis* at from 590 to 2,220, or for the four species areas of distribution in depth ranging from 2,782, 2,561, 2,502, and 2,000 meters. We might multiply such examples, but those cited appear to be sufficient to show that the organism of fishes of certain depths is such that it is capable of enduring enormous pressures without suffering therefrom. The forms of the fishes just mentioned have nothing peculiar to attract one's attention and to distinguish them from those proper to the fishes that live near the surface. Their dental system is highly developed, and this feature shows that they are carnivorous (Fig. 3). Carnivorous characters are found in all fishes that live continually at depth below 660 meters. This is due to the fact that, in consequence of the absence of light, vegetation rapidly disappears at the bottom of the sea, and therefore all species that do not rise to within 150 meters of the surface (the point where the last algæ are met with) are forced to hunt animal food.

In Fig. 4 we represent one of these species of fishes—the *Macrurus globiceps*—whose area of distribution, according to the depth, is included between 1,400 and 3,000 meters. This new species has been described by Mr. L. Vaillant.

Although the fishes that occasionally visit great depths do not present abnormal forms, the same is not the case with those that continuously inhabit great ocean depths. There is nothing surprising in this fact, since the organism of these animals has had to undergo profound modifications in order to adapt itself to very peculiar conditions of existence. The influences to which fishes at great depths are submitted are multiple. Where they dwell both light and vegetation are absent; proceeding to a certain depth, the temperature of the medium in which they are placed is equalized; and finally the water at the depth at which they live is always calm. The modifications succeeding these different actions have an influence upon the structure of the tissues, the increase of the eyes, the development of the organs of touch, and the coloration.

Besides, in fishes at great depths there are found organs that are unknown in ordinary ones, and the function of which consists in the disengagement of phosphorescent light, thus making up for the light of the sun, which is entirely absent.

The changes that the tissues have undergone are seen in the structure of the skin, muscles, and bones. The skin is thin and always deprived of bright colors, the tints that it exhibits varying from grayish to a velvety black (Fig. 5). The scales, often of reduced size, are scarcely fixed, and the friction that they undergo during the hauling up of the drag-net suffices to detach nearly all of them. The muscles are of a soft consistency, and constitute a food that is not very agreeable and that is deprived of flavor. The bones are composed of a friable tissue, and their interior is spongy.

In fishes that live continuously at depths to which a few rays of light still penetrate the eyes become very large, in order to offer a greater sensitive surface. This fact recalls that which we observe in crepuscular birds, whose organs of vision are likewise highly developed. In fishes living at very great depths this increase in the size of the eyes is not observed, these organs preserving their normal size, and possessing nothing peculiar either in their arrangement or structure. Their operation in a completely dark medium seems at first sight incomprehensible. It is explained however when we find that the animals to which they belong possess phosphorescent organs, or else are covered with a luminous mucus capable of projecting light to quite a great distance.

The phosphorescence that fishes at great depths possess must serve them on the one hand to guide them and on the other to attract prey to them. It subserves the same purpose to them in the latter case that a torch does in the hands of a fisherman.[1] This fact has long been known as regards

surface fish that hunt at night. Thus, Bennet has made known a species of shark remarkable for a brilliant green phosphorescence that is given off from the entire lower part of its body. This learned zoologist, having one day carried an individual of this species that he had captured into a room, the latter was immediately filled with light disengaged from the animal's body. The light was increased neither by motion nor friction. After the shark's death the light from the belly first disappeared, and the jaws and fins were the last parts to remain phosphorescent. The different species of sharks which are found only at depths of two thousand meters, and several examples of which, taken off the coast of Portugal, figure at the exhibition, must, like the fish Bennet speaks of, use the light which they emit for attracting animals that serve them as food. What is the origin of this mucus that possesses so brilliant an illuminating power? It appears that it must be referred to grandular organs that are distributed along the sides and the tail, and sometimes, but more rarely, upon the back. But, in addition to these grandular organs, there exist in certain fishes apparatus of an entirely different nature that serve to emit light. These organs consist of a sort of translucent biconvex lens that in front closes a chamber filled with a transparent liquid. This chamber is lined with a blackish membrane (composed of hexagonal cells) which recalls the retina of the eye, and which is connected with the nervous branches. The phosphorescent apparatus thus formed are placed either under the eyes or upon the lateral portions of the body. Some zoologists consider these organs, because of the quasi-retinal membrane that lines them, and of its connection with the nerves, as a sort of accessory eyes. Such an opinion seems to be very difficult to admit, when we consider the normal development of the eyes, and it seems much more rational to suppose that they serve simply for producing a light, which, thanks to the sort of lens that limits them in front, may be condensed upon a given point.

Deep sea fishes appear to move very little. They evidently live buried in the slime, since we constantly remark upon those that are taken particles of mud incrusted upon some parts of their body. A few rays of their fins are often diverted from their habitual function and become organs of touch. One of the most remarkable of such deviations is offered by a fish that we took off the coast of Africa—the *Melanocetus Johnsoni*. In this animal, which was unknown except from a single individual found dead upon the surface of the sea in the vicinity of Madeira, the first ray of the dorsal fin develops and forms a true tactile appendage that must serve the same purpose as that of the baudroie. In this latter fish there likewise exists a tentacle placed at the extremity of the first ray of the dorsal fin. The baudroie lives amid sand or mud, out of which it hollows a cavity by means of its fins, and buries itself therein so as to allow only the extreme upper parts of its body to project. It incessantly agitates its tentacle, which serves it as a bait to attract fishes that it afterward voraciously seizes. Other very singular transformations of the rays of the fins into tactile organs may be recognized in different fishes captured by the Talisman. Of these we must especially mention the *Bathypterus*.

Among the most singular tactile organs that we have observed in fishes must be cited that of the *Eustomias obscurus*, which is situated immediately beneath the mouth. This new genus of fishes is shown in Fig. 5. Our first engraving represents the *Neostoma batyphillum*, found at a depth of 2,220 meters.

One of the very remarkable characters proper to fishes living at great depths in the enormous development of the mouth and stomach. In the *Melanocetus* and *Chiasmodus* the capacity of this organ is such that it is capable of containing prey whose bulk is double that of the body of the fish to which it belongs. As for the proportions assumed by the buccal cavity, the maximum development that they seem to be capable of acquiring is exhibited by the *Eurypharynx pelecanoides*.

One of the most interesting questions concerning the distribution of fishes in the sea is that which relates to the maximum depth at which these animals may be met with. During the cruise of the Tailsman the fish taken at the greatest depth (4,235 meters) was the *Bythytes crassus*. During the expedition of the Challenger, a fish, the *Bathyophis ferox*, was captured at a depth of 5,019 meters.—*H. Mihol, in La Nature.*

THE SIX GATEWAYS OF KNOWLEDGE.*

By Prof. SIR WILLIAM THOMSON.

I THANK you most warmly for the honor you have done me in electing me to be your president. I value the honor very highly; but when I look at the list of the distinguished men who have preceded me in the office, I feel alarmed at the responsibility I have undertaken. A very pleasing duty, however, has been already performed in the interesting and not onerous function we have now gone through. I would gladly speak on the several subjects, for merit in the study of which these prizes have been awarded; but I am afraid that if I were to do so, it would be more for my own gratification than for your pleasure and profit, and I feel that I shall best consult your wishes in passing on at once to the subject of the address which it becomes my duty to give.

The title of the subject upon which I am going to speak this evening might be—if I were asked to give it a title—"The Six Gateways of Knowledge." I feel that the subject I am about to bring before you is closely connected with the studies for which the several prizes have been given. The question I am going to ask you to think of is: What are the means by which the human mind acquires knowledge of external matter?

John Bunyan likens the human soul to a citadel on a hill, self-contained, having no means of communication with the outer world, except by five gates—Eye Gate, Ear Gate, Mouth Gate, Nose Gate, and Feel Gate. Bunyan clearly was in want of a word here. He uses "feel" in the sense of "touch," a designation which to this day is so commonly used that I can scarcely accuse it of being incorrect. At the same time the more correct and distinct designation undoubtedly is, the sense of touch. The late Dr. George Wilson, first Professor of Technology in the University of Edinburgh, gave, sometime before his death, a beautiful little book under the title of "The Five Gateways of Knowledge," in which he quotes John Bunyan in the manner I have indicated to you. But I have said six gateways of knowledge, and I must endeavor to justify this saying. I am going to try to prove to you that we have six senses—that if we are to number the senses at all, we must make them six.

The only census of the senses, so far as I am aware, that ever made them more than five before was the Irishman's reckoning of seven senses. I presume the Irishman's seventh sense was common sense; and I believe that the large possession of that virtue by my countrymen—I speak as an Irishman—I say the large possession of the seventh sense, which I believe Irishmen have, and the exercise of it, will do more to alleviate the woes of Ireland than even the removal of the melancholy ocean which surrounds its shores. Still I cannot scientifically see how we can make more than six senses. I shall, however, should time permit, return to this question of a seventh sense, and I shall endeavor to throw out suggestions toward answering the question—Is there, or is there not, a magnetic sense? It is possible that there is, but facts and observations so far give us no evidence that there is a magnetic sense.

The six senses that I intend to explain, so far as I can, this evening, are, according to the ordinary enumeration, the sense of sight, the sense of hearing, the sense of smell, the sense of taste, and the sense of touch, divided into two departments. A hundred years ago Dr. Thomas Reid, Professor of Moral Philosophy in the University of Glasgow, pointed out that there was a broad distinction between the sense of roughness or of resistance, which was possessed by the hand, and the sense of heat. Reid's idea has not I think been carried out so much as it deserves. We do not, I believe, find in any of the elementary treatises on natural philosophy, or in the physiologists' writings upon the senses, a distinct reckoning of six senses. We have a great deal of explanation about the muscular sense, and the tactile sense; but we have not a clear and broad distinction of the sense of touch into two departments, which seems to me to follow from Dr. Thomas Reid's way of explaining the sense of touch, although he does not himself distinctly formulate the distinction I am now going to explain.

The sense of touch, of which the organ commonly considered is the hand, but which is possessed by the whole sensitive surface of the body, is very distinctly a double quality. If I touch any object, I perceive a complication of sensations, I perceive a certain sense of roughness, but I also perceive a very distinct sensation which is not of roughness, or of smoothness. There are two sensations here, let us try to analyze them. Let me dip my hand into this bowl of hot water. The moment I touch the water, I perceive a very distinct sensation, a sensation of heat. Is that a sensation of roughness, or of smoothness? No. Again, I dip my hand into this basin of iced water. I perceive a very distinct sensation. Is this a sensation of roughness, or of smoothness? No. Is this comparable with that former sensation of heat? I say yes. Although it is opposite, it is comparable with the sensation of heat. I am not going to say that we have two sensations in this department—a sensation of heat, and a sensation of cold. I shall endeavor to explain that the perceptions of heat and of cold are perceptions of different degrees of one and the same quality, but that that quality is markedly different from the sense of roughness. Well, now, what is this sense of roughness? It will take me some time to explain it fully. I shall therefore say in advance that it is a sense of force; and I shall tell you in advance, before I justify completely what I have to say, that the six senses, regarding which I wish to give some explanation, are: the sense of sight, the sense of hearing, the sense of taste, the sense of smell, the sense of heat, and the sense of force. The sense of force is the sixth sense; or the senses of heat and of force are the sense of touch divided into two, to complete the census of six that I am endeavoring to demonstrate.

Now, I have hinted at a possible seventh sense—a magnetic sense—and though out of the line I propose to follow, and although time is precious, and does not permit of much digression, I wish just to remove the idea that I am in any way suggesting anything toward that wretched superstition of animal magnetism, and table-turning, and spiritualism, and mesmerism, and clairvoyance, and spirit-rapping, of which we have heard so much. There is no seventh sense of the mystic kind. Clairvoyance, and the like, are the result of bad observation chiefly, somewhat mixed up, however, with the effects of willful imposture, acting on an innocent, trusting mind. But if there is not a distinct magnetic sense, I say it is a very great wonder that there is not.

Many present know all about magnetism. A very large number of pupils have gained an immense amount of valuable knowledge in various subjects, from the classes carried on nightly within the walls of the Birmingham and Midland Institute; and I can see from the prizes that have been awarded, and that I have just now had the pleasure of distributing for excellence and proficiency in this department, that many have learned of magnetism. I had the pleasure of seeing the class-rooms this morning, and I wished I could be in them in the evening to see the studies as carried on in them every evening. Well, now, the study of magnetism is the study of a very recondite subject. We all know a little about the mariner's compass, the needle pointing to the north, and so on; but not many of us have gone far into the subject, and not many of us understand all the recent discoveries in electro-magnetism. I could wish, had I the apparatus here, and if you would allow me, to show you an experiment in magnetism. If we had before us a powerful magnet, or say the machine that is giving us this beautiful electric light by which the hall is illuminated, it, serving to excite an electro-magnet, would be one part of our apparatus; the other part would be a piece of copper. Suppose then we had this apparatus, I would show you a very wonderful discovery made by Faraday, and worked out admirably by Foucault, an excellent French experimenter. I have said that one part of this apparatus would be a piece of copper, but silver would answer as well. Probably no other metal than copper or silver—certainly no other one, of all the metals that are well known, and obtainable for ordinary experiments—possesses, and no other metal or substance, whether metallic or not, is known to possess, in anything like the same degree as copper and silver, the quality I am now going to call attention to.

The quality I refer to is "electric conductivity," and the result of that quality in the experiment I am now going to describe is, that a piece of copper or a piece of silver, let fall between the poles of a magnet, will fall down slowly as if it were falling through mud. I take this body, and let it fall. Many of you here will be able to calculate what fraction of a second it takes to fall one foot. If I took this piece of of copper, placed it just above the space between the poles of a powerful electro-magnet and let it go, you would see it fall slowly down before you; it would perhaps take a quarter of a minute to fall a few inches.

This experiment was carried out in a most powerful manner by Lord Lindsay (now Lord Crawford), assisted by Mr. Cromwell F. Varley. Both of those eminent men desired to investigate the phenomena of mesmerism, which had been called animal magnetism; and they very earnestly set to work to make a real physical experiment. They asked themselves, Is it conceivable that, if a piece of copper can scarcely move through the air between the poles of an electro-magnet, a human being or other living creature placed there would experience no effect? Lord Lindsay got an enormous electro-magnet made, so large that the head of any person wishing to try the experiment could get well between the poles, in a region of excessively powerful magnetic force. What was the result of the experiment? If I were to say *nothing*, I should do it scant justice. The result was marvelous, and the marvel is that nothing was perceived. Your head, in a space through which a piece of copper falls as if through mud, perceives nothing. I say this is a very great wonder; but I do not admit, I do not feel, that the investigation of the subject is completed. I cannot think that the quality of matter in space which produces such a prodigious effect upon a piece of metal can be absolutely without any—it is certainly not without any—effect whatever on the matter of a living body; and that it can be absolutely without any effect whatever *perceptible* on the matter of a living body placed there seems to me not proved even yet, although nothing has been found. It is so marvelous that there should be no effect at all, that I do believe and feel that the experiment is worth repeating; and that it is worth examining, whether or not an exceedingly powerful magnetic force has any perceptible effect upon a living vegetable or animal body. I spoke then of a seventh sense. I think it just possible that there may be a magnetic sense; I think it possible that an exceeding powerful magnetic effect may produce a sensation that we cannot compare with heat or force or any other sensation.

Another question that often occurs is: "Is there an electric sense?" Has any human being a perception of electricity in the air? Well, somewhat similar proposals for experiment might, perhaps, be made with reference to electricity; but there are certain reasons, that would take too long for me to explain, that prevent me from placing the electric force at all in the same category with magnetic force. There would be a surface action that would annul practically the force in the interior, there would be a definite sensation which we could distinctly trace to the sense of touch.

Any one putting his hand, or his face, or his hair, in the neighborhood of an electric machine perceives a sensation, and on examining it he finds that there is a current of air blowing, and his hair is attracted; and if he puts his hand too near, he finds that there are sparks passing between his hand or face and the machine; so that, before we come to any subtle question of a possible sense of electric force, we have distinct mechanical agencies which give rise to senses of temperature and force; but that this mysterious, wonderful, magnetic force, due, as we know, to rotations of the molecules, could be absolutely without effect—without perceptible effect—on animal economy seems a very wonderful result, and at all events it is a subject deserving careful investigation. I hope no one will think I am favoring the superstition of mesmerism in what I have said.

I intend to explain a little more fully our perceptions in connection with the double sense of touch—the sense of temperature and the sense of force—should time permit before I conclude. But I must first say something of the other senses, because if I speak too much about the senses of force and heat no time will be left for any of the others.

Well, now, let us think what it is we perceive in the sense of hearing. Acoustics is one of the studies of the Birmingham and Midland Institute, of which we have heard many times this evening. Acoustics is the science of hearing. And what is hearing? Hearing is perceiving something with the ear. What is it we perceive with the ear? It is something we can also perceive without the ear; something that the greatest master of sound, in the poetic and artistic sense of the word at all events, that ever lived—Beethoven—for a great part of his life could not perceive with his ear at all. He was deaf for a great part of his life, and during that period were composed some of his grandest musical compositions, and without the possibility of his ever hearing them by ear himself; for his hearing by ear was gone from him forever. But he used to stand with a stick pressed against the piano and touching his teeth, and thus he could hear the sounds that be called forth from the instrument. Hence, besides the Ear Gate of John Bunyan, there is another gate or access for the sense of hearing.

What is it that you perceive ordinarily by the ear—that a healthy person, without the loss of any of his natural organs of sense, perceives with his ear, but which can otherwise be perceived, although not so satisfactorily or completely? It is distinctly a sense of varying pressure. When the barometer rises, the pressure on the ear increases; when the barometer falls, that is an indication that the pressure on the ear is diminishing.

Well, if the pressure of air were suddenly to increase and diminish, say in the course of a quarter of a minute—suppose in a quarter of a minute the barometer rose one-tenth of an inch and fell again, would you perceive anything? I doubt it; I do not think you would. If the barometer were to rise two inches, or three inches, or four inches, in the course of half a minute, most people would perceive it. I say this as a result of observation, because people going down in a diving bell have exactly the same sensation as they would experience if from some unknown cause the barometer quickly, in the course of half a minute, were to rise five or six inches—far above the greatest height it ever stands at in the open air.

Well, now, we have a sense of barometric pressure, but we have not a continued indication that allows us to perceive the difference between the high and low barometer. People living at great altitudes—up several thousand feet above the level of the sea, where the barometer stands several inches lower than at sea-level—feel very much as they would do at the surface of the sea, so far as any sensation of pressure is concerned. Keen mountain air feels different from air in lower places, partly because it is colder and drier, but also because it is less dense, and you must breathe more of it to get the same quantity of oxygen into your lungs to perform those functions which the students of the institute who study animal physiology—and I understand there are a large number—will perfectly understand. The effect of the air in the lungs—the function it performs—depends chiefly on the oxygen taken in. If the air has only three-quarters of the density it has in our ordinary atmosphere here, then one and one-third times as much must be inhaled, to produce the same oxidizing effect on the blood and the same general effect in the animal economy; and in that way undoubtedly mountain air has a very different effect on living creatures from the air of the plains. This effect is distinctly perceptible in its relation to health.

But I am wandering from my subject, which is the consideration of the changes of pressure comparable with those that produce sound. A diving bell allows us to perceive a sudden increase of pressure, but not by the ordinary sense of touch. The hand does not perceive the difference between 15 lb. per square inch pressing it all around and 17 lb., or 18 lb,, or 20 lb., or even 30 lb. per square inch, as is

* An address at the Midland Institute, Birmingham, October 3, 1883, by Prof. Sir William Thomson, LL.D., F.R.S., president.

experienced when you go down in a diving bell. If you go down five and a half fathoms in a diving bell, your hand is pressed all round with a force of 30 lb. to the square inch; but yet you do not perceive any difference in the sense of force, any perception of pressure.

What you do perceive is this: behind the tympanum is a certain cavity filled with air, and a greater pressure on one side of the tympanum than on the other gives rise to a painful sensation, and sometimes produces rupture of it in a person going down in a diving bell suddenly. The remedy for the painful sensation thus experienced, or rather I should say its prevention, is to keep chewing a piece of hard biscuit or making believe to do so. If you are chewing a hard biscuit, the operation keeps open a certain passage, by which the air pressure gets access to the inside of the tympanum, and balances the outside pressure and thus prevents the painful effect. This painful effect on the ear experienced by going down in a diving bell is simply because a certain piece of tissue is being pressed more on one side than on the other, and when we get such a tremendous force on a delicate thing like the tympanum, we may experience a great deal of pain, and it may be dangerous; indeed, it is dangerous, and produces rupture or damage to the tympanum unless means be adopted for obviating the difference in the pressures; but the simple means I have indicated are, I believe, with all ordinary healthy persons, perfectly successful.

I am afraid we are no nearer, however, to understanding what it is we perceive when we hear. To be short, it is simply this: it is exceedingly sudden changes of pressure acting on the tympanum of the ear, through such a short time and with such moderate force as not to hurt it, but to give rise to a very distinct sensation, which is communicated through a train of bones to the auditory nerve. I must merely pass over this; the details are full of interest, but they would occupy us far more than an hour if I entered upon them at all. As soon as we get to the nerves and the bones, we have gone beyond the subject I proposed to speak upon. My subject belongs to physical science—what is called in Scotland, Natural Philosophy. Physical science refers to dead matter, and I have gone beyond the range whenever I speak of a living body; but we must speak of a living body in dealing with the senses as the means or perceiving—as the means by which, in John Bunyan's language, the soul in its citadel acquires a knowledge of external matter. The physicist has to think of the organs of sense, merely as he thinks of the microscope; he has nothing to do with physiology. He has a great deal to do with his own eyes and hands, however, and must think of them if he would understand what he is doing, and wishes to get a reasonable view of the subject, whatever it may be, which is before him in his own department.

Now, what is the external object of this internal action of hearing and perceiving sound? The external object is a change of pressure of air. Well, how are we to define a sound simply? It looks a little like a vicious circle, but indeed it is not so, to say it is sound if we call it a sound—if we perceive it as sound, it is sound. Any change of pressure which is so sudden as to let us perceive it as sound is a sound. There [giving a sudden clap of the hands]—that is a sound. There is no question about it—nobody will ever ask, Is it a sound or not? It is sound if you hear it. If you do not hear it, it is not to you a sound. That is all I can say to define sound. To explain what it is, I can say, it is change of pressure, and it differs from a gradual change of pressure as seen on the barometer only in being more rapid, so rapid that we perceive it as a sound. If you could perceive by the ear that the barometer has fallen two-tenths of an inch today, that would be sound. But nobody hears by his ear that the barometer has fallen, and so he does not perceive the fall as a sound. But the same difference of pressure coming on us suddenly—a fall of the barometer, if by any means it could happen, amounting to a tenth of an inch, and taking place in a thousandth of a second—would affect us quite like sound. A sudden rise of the barometer would produce a sound analogous to what happened when I clapped my hands. What is the difference between a noise and a musical sound? Musical sound is a regular and periodic change of pressure. It is an alternate augmentation and dimunition of air pressure, occurring rapidly enough to be perceived as a sound, and taking place with perfect regularity, period after period. Noises and musical sounds merge into one another. Musical sounds have a possibility at least of sometimes ending in a noise, or tending too much to a noise, to altogether please a fastidious musical ear. All roughness, irregularity, want of regular, smooth periodicity, has the effect of playing out of tune, or of music that is so complicated that it is impossible to say whether it is in tune or not.

But now, with reference to this sense of sound, there is something I should like to say as to the practical lesson to be drawn from the great mathematical treatises which were placed before the British Association, in the addresses of its president, Prof. Cayley, and of the president of the mathematical and physical section, Prof. Henrici. Both of these professors dwelt on the importance of graphical illustration, and one graphical illustration of Prof. Cayley's address may be adduced in respect of this very quality of sound. In the language of mathematics we have just "one independent variable" to deal with in sound, and that is air pressure. We have not a complication of motions in various directions. We have not the complication that we shall have to think of presently, in connection with the sense of force, complication as to the place of application and the direction of the force. We have not the infinite complications we have in some of the other senses, notably smell and taste. We have distinctly only one thing to consider, and that is air pressure or the variation of air pressure. Now, when we have one thing that varies, that, in the language of mathematics, is "one independent variable." Do not imagine that mathematics is harsh, and crabbed, and repulsive to common sense. It is merely the etherealization of common sense. The function of one independent variable that you have here to deal with is the pressure of air on the tympanum. Well, now, in a thousand counting houses and business houses in Birmingham, and London, and Glasgow, and Manchester, a curve, as Prof. Cayley pointed out, is regularly used to show to the eye a function of one independent variable. The function of one independent variable most important in Liverpool perhaps may be the price of cotton. A curve showing the price of cotton, rising when the price of cotton is high, and sinking when the price of cotton is low, shows all the complicated changes of that independent variable to the eye. And so in the Registrar-General's tables of mortality, we have curves showing the number of deaths from day to day—the painful history of an epidemic, shown in a rising branch, and the long, gradual talus in a falling branch of the curve, when the epidemic is overcome, and the normal state of health is again approached. All that is shown to the eye; and one of the most beautiful results of mathematics is the means of showing to the eye the law of variation, however complicated, of one independent variable. But now for what really to me seems a marvel of marvels: think what a complicated thing is the result of an orchestra playing—a hundred instruments and two hundred voices singing in chorus accompanied by the orchestra. Think of the condition of the air, how it is lacerated sometimes in a complicated effect. Think of the smooth, gradual increase and diminution of pressure—smooth and gradual, though taking place several hundred times in a second—when a piece of beautiful harmony is heard! Whether, however, it be the single note of the most delicate sound of a flute, or the purest piece of harmony of two voices singing perfectly in tune; or whether it be the crash of an orchesta, and the high notes, sometimes even screechings and tearings of the air, which you may hear fluttering above the sound of the chorus—think of all that, and yet that is not too complicated to be represented by Prof. Cayley, with a piece of chalk in his hand, drawing on the blackboard a single line. A single curve, drawn in the manner of the curve of prices of cotton, describes all that the ear can possibly hear, as the result of the most complicated musical performance. How is one sound more complicated than another? It is simply that in the complicated sound the variations of our one independent variable, pressure of air, are more abrupt, more sudden, less smooth, and less distinctly periodic, than they are in the softer, and purer, and simpler sound. But the superposition of the different effects is really a marvel of marvels; and to think that all the different effects of all the different instruments can be so represented! Think of it in this way. I suppose everybody present knows what a musical score is; you know, at all events, what the notes of a hymn tune look like, and can understand the like for a chorus of voices and accompanying orchestra—a "score" of a whole page with a line for each instrument, and with perhaps four different lines for four voice parts. Think of how much you have to put down on a page of manuscript or print, to show what the different performers are to do. Think, too, how much more there is to be done than anything the composer can put on the page. Think of the expression which each player is able to give, and of the difference between a great player on the violin and a person who simply grinds successfully through his part; think, too, of the difference in singing, and of all the expression put into a note or a sequence of notes in singing that cannot be written down. There is, on the written or printed page, a little wedge showing a diminuendo, and a wedge turned the other way showing a crescendo, and that is all the musician can put on paper to mark the difference of expression which is to be given. Well, now, all that can be represented by a whole page or two pages of orchestral score, as the specification of the sound to be produced in say ten seconds of time is shown to the eye with perfect clearness by a single curve on a ribbon of paper a hundred inches long. That to my mind is wonderful proof of the potency of mathematics. Do not let any student in this institute be deterred for a moment from the pursuit of mathematical studies by thinking that the great mathematicians get into the realm of four dimensions, where you cannot follow them. Take what Prof. Cayley himself in his admirable address, which I have already referred to, told us of the beautiful and splendid power of mathematics for etherealizing and illustrating common sense, and you need not be disheartened in your studies of mathematics, but may rather be reinvigorated when you think of the power which mathematicians, devoting their whole lives to the study of mathematics, have succeeded in giving to that marvelous science.

The sense of sight may be compared to the sense of sound in this respect. I spoke of the sense of sound being caused by rapid variations of pressure. I had better particularize, and say how rapid must be the alterations from greatest pressure to least, and back to greatest, and how frequently must that period occur, to give us the sound of a musical note. If the barometer varies once a minute, you would not perceive that as a musical note. But suppose by any mechanical action in the air, you could cause the barometric pressure—the air pressure—to vary much more rapidly. That change of pressure which the barometer is not quick enough to show to the eye, the ear hears as a musical sound if the period recurs twenty times per second. If it recurs twenty, thirty, forty, or fifty times per second, you hear a low note. If the period is gradually accelerated, you hear the low note gradually rising, becoming higher and higher, more and more acute, and if it gets up to 256 periods per second, we have a certain note called C in the ordinary musical notation. I believe I describe it correctly as the low note C of the tenor voice—the gravest C that can be made by a flute. The note of a two-foot organ pipe open at both ends has 256 periods per second. Go on higher and higher to 512 periods per second, and you have the C above that—the chief C of the soprano voice. Go above that to 1,024, you get an octave higher. You get an octave higher always by doubling the number of vibrations per second, and if you go on till you get up to about 5,000 or 6,000 or 10,000 periods per second, the note becomes so shrill that it ceases to excite the human ear, and you do not hear it any longer. The highest note that can be perceived by the human ear seems to be something like 10,000 periods per second. I say, "something like," because there is no very definite limit. Some ears cease to hear a note becoming shriller and shriller before other ears cease to hear it; and therefore I can only say in a very general way, that something like 10,000 periods per second is about the shrillest note the human ear is adapted to hear. We may define musical notes, therefore, as changes of pressure of the air, regularly alternating in periods which lie between 20 and 10,000 per second.

Well, now, are there vibrations of thirty or forty or fifty or a hundred thousand or a million of periods per second in air, in elastic solids, or in any matter affecting our sense? We have no evidence of the existence in matter of vibrations of very much greater frequency than 10,000 or 20,000 or 30,000 per second, but we have no reason to deny the possibility of such vibrations existing, and having a large function to perform in nature. But when we get to some degree of frequency that I cannot put figures upon, to something that may be measured in millions, if not in hundreds of thousands of vibrations per second, we have not merely passed the limits of the human ear to hear, but we have passed the limits of matter, as known to us, to vibrate. Vibrations transmitted as waves through steel, or air, or water, cannot be more frequent than a certain number, which I cannot now put a figure to, but which, I say, may be reckoned in hundreds of thousands or a few millions per second.

But now let us think of light. Light we know to be an influence on the retina of the eye, and through the retina on the optic nerve; an influence dependent on vibrations whose frequency is something between 400 million millions per second and 800 million millions per second. Now, we have a vast gap between 400 per second, the sound of a rather high tenor voice, and 400 million millions per second, the number of vibrations corresponding to dull red light—the gravest red light of the prismatic spectrum. Take the middle of the spectrum—yellow light—the period of the vibrations there is in round numbers 500 million millions per second. In violet light we have 800 million millions per second. Beyond that we have something that the eye scarcely perceives—does not perceive at all perhaps—but which I believe it does perceive, though not vividly; we have the ultra-violet rays, known to us chiefly by their photographic effect, but known also by many other wonderful experiments, that within the last thirty years have enlarged our knowledge of light to a most marvelous degree. We have invisible rays of light made visible by letting them fall on a certain kind of glass, glass tinged with uranium—that yellowish green glass, sometimes called canary glass or chameleon glass. Uranium glass has a property rendering visible to us invisible rays. You may hold a piece of uranium glass in your hand, illuminated by this electric light, or by a candle, or by gas light, or hold it in the prismatic spectrum of white light, and you see it glowing according to the color of the light which falls upon it; but place it in the spectrum beyond the visible violet end, where without it you see nothing, where a piece of chalk held up seems quite dark, and the uranium glass glows with a mysterious altered color of a beautiful tint, revealing the presence of invisible rays, by converting them into rays of lower period, and so rendering them visible to the eye. The discovery of this property of uranium glass was made by Prof. Stokes, and the name of fluorescence, from fluor spar, which he found to have the property, was given to it. It has since been discovered that fluorescence and phosphorescence are continuous, being extremes of the same phenomenon. I suppose most persons here present know the luminous paint made from sulphides of calcium and other materials, which, after being steeped in light for a certain time, keep on for hours giving out light in the darkness. Persistence in emission of light after the removal of the source, which is the characteristic of those phosphorescent objects, is manifested also, as Edmund Becquerel has proved by the uranium glass, and thus Stokes' discovery of fluorescence comes to be continuous with the old known phenomenon of phosphorescence, to which attention seems to have been first called scientifically by Robert Boyle about 200 years ago.

There are other rays, that we do not perceive in any of these ways, but that we do perceive by our sense of heat: heat rays, as they are commonly called. But in truth all rays that we call light have heating effect. Radiant heat and light are one and indivisible. There are not two things, radiant heat and light: radiant heat is identical with light. Take a black hot kettle into a dark room, and look at it. You do not see it. Hold your face or your hand near it, and you perceive it by what Bunyan would have called Feel Gate; only now we apply the word feeling to other senses as well as Touch. You perceive it before you touch it. You perceive it with the back of your hand, or the front of your hand; you perceive it with your face, yes, and with your eye, but you do not see it. Well, now, must I justify the assertion that it is not light? You say it is not light, and it is not so to you, if you do not see it. There has been a good deal of logic-chopping about the words here; we seem to define in a vicious circle. We may begin by defining light—"It is light if you see it as light; it is not light if you do not see it." To save circumlocution, we shall take things in that way. Radiant heat is light if we see it, it is not light if we do not see it. It is not that there are two things; it is that radiant heat has differences of quality. There are qualities of radiant heat that we can see, and if we see them we call them light; there are qualities of radiant heat we cannot see, and if we cannot see them we do not call them light, but still call them radiant heat; and that on the whole seems to me to be the best logic for this subject.

By the bye, I don't see Logic among the studies of the Birmingham and Midland Institute. Logic is to language and grammar what mathematics is to common sense; logic is etherealized grammar. I hope the advanced student in grammar and Latin and Greek, who needs logic perhaps as much as, perhaps more than, most students of science and modern languages, will advance to logic, and consider logic as the science of using words, to lead him to know exactly what he means by them when he uses them. More ships have been wrecked through bad logic than by bad seamanship. When the captain writes down in his log—I don't mean a pun here, log has nothing to do with logic—the ship's place is so-and-so, he means that it is the most probable position—the position which, according to previous observations, he thinks is the most probable. After that, supposing no sights of sun or stars or land to be had, careful observation of speed and direction shows, by a simple reckoning (called technically the dead reckoning), where the ship is next day. But sailors too often forget that what they put down in the log was not the ship's place, but what to their then knowledge was the most probable position of the ship, and they keep running on as if it was the true position. They forget the meaning of the very words in which they have made their entry in the log, and through that bad logic more ships have been run on the rocks than by any other carelessness or bad seamanship. It is bad logic that leads to trusting to the dead reckoning, in running a course at sea; and it is that bad logic which is the cause of those terribly frequent wrecks; of steamers, otherwise well conducted, in cloudy but perfectly fine weather, running on rocks at the end of a long voyage. To enable you to understand precisely the meaning of your result when you make a note of anything about your own experience or experiments, and to understand precisely the meaning of what you write down, is the province of logic. To arrange your record in such a manner that if you look at it afterward it will tell you what it is worth, and neither more nor less, is practical logic; and if you exercise that practical logic, you will find benefits that are too obvious if you only think of any scientific or practical subject with which you are familiar.

There is danger then of a bad use of words, and hence of bad reasoning upon them, in speaking of light and radiant heat; but if we distinctly define light as that which we consciously perceive as light—without attempting to define consciousness, because we cannot define consciousness any more than we can define free will—we shall be safe.

There is no question that you see the thing; if you see it, it is light. Well, now, when is radiant heat light? Radiant heat is light when its frequency of vibration is between 400 million millions per second and 800 million millions per second. When its frequency is less than 400 million millions per second it is not light; it is invisible "infra-red" radiant heat. When its frequency is more than 800 million millions per second, it is not light if we cannot see it; it is invisible ultra-violet radiation, truly radiant heat, but it is not so commonly called radiant heat because its heating effect is known rather theoretically than by sensory perception, or thermometric or thermoscopic indications. Observations which have been actually made by Langley and by Abney on

radiant heat take us down about three octaves below violet, and we may hope to be brought considerably lower still by future observation. We know at present in all about four octaves—that is, from one to two, two to four, four to eight, eight to sixteen, hundred million millions—of radiant heat. One octave of radiant heat is perceptible to the eye as light, the octave from 400 million millions to 800 million millions. I borrow the word octave from music, not in any mystic sense, nor as indicating any relation between harmony of colors and harmony of sound. No relation exists between harmony of sound and harmony of colors. I merely use the word octave as a brief expression for any range of frequencies lying within the ratio of one to two. If you double the frequency of a musical note, you raise it an octave; in that sense I used the word for the moment in respect to light, and in no other sense. Well, now, think what a tremendous chasm there is between the 100 million millions per second, which is about the gravest hitherto discovered note of invisible radiant heat, and the 10,000 per second, the greatest number of vibrations in sound. This is an unknown province of science; the investigation of vibrations between those two limits is perhaps one of the most promising provinces of science for the future investigator.

In conclusion, I wish to bring before you the idea that all the senses are related to force. The sense of sound, we have seen, is merely a sense of very rapid changes of air pressure (which is force) on the drum of the ear. I have passed merely by name over the senses of taste and smell. I may say they are chemical senses. Taste common salt and taste sugar—you tell in a moment the difference. The perception of that difference is a perception of chemical quality. Well, there is a subtle molecular influence here, due to the touch of the object on the tongue or the palate, and producing a sensation which is a very different thing from the ordinarily reckoned sense of touch, in the case now considered, telling only of roughness and of temperature. The most subtle of our senses perhaps is sight; next come smell and taste. Prof. Stokes recently told me that he would rather look upon taste and smell and sight as being continuous because they are all molecular—they all deal with properties of matter, not in the gross, but molecular actions of matter; he would rather group those three together than he would couple any of them with any of the other senses. It is not necessary, however, for us to reduce all the six senses to one, but I would just point out that they are all related to force. Chemical action is a force, tearing molecules apart, throwing or pushing them together; and our chemical sense or senses may therefore so far at least be regarded as concerned with force. That the senses of smell and taste are related to one another seems obvious; and if physiologists would pardon me, I would suggest that they may, without impropriety, be regarded as extremes of one sense. This at all events can be said of them, they can be compared—which cannot be said of any other two senses. You cannot say that the shape of a cube, or the roughness of a piece of loaf sugar or sandstone, is comparable with the temperature of hot water, or is like the sound of a trumpet, or that the sound of a trumpet is like scarlet, or like a rocket, or like a blue light signal. There is no comparability between any of these perceptions. But if any one says, "That piece of cinnamon tastes like its smell," I think he will express something of general experience. The smell and taste of pepper, nutmeg, cloves, cinnamon, vanilla, apples, strawberries, and other articles of food, particularly spices and fruits, have very marked qualities, in which the taste and the smell seem essentially comparable. It does seem to me, although anatomists distinguish between them, because the sensory organs concerned are different and because they have not discovered a continuity between these organs, we should not be philosophically wrong in saying that smell and taste are extremes of one sense—one kind of perceptivity—a sense of chemical quality materially presented to us.

Now, sense of light and sense of heat are very different; but we cannot define the difference. You perceive the heat of a hot kettle—how? By its radiant heat, against the face—that is one way. But there is another way, not by radiant heat, of which I shall speak later. You perceive by vision, but still in virtue of radiant heat, a hot body, if illuminatad by light, or if hot enough to be self-luminous, red-hot or white-hot, you see it; you can both see a hot body, and perceive it by its heat, otherwise than by seeing it. Take a piece of red-hot cinder with the tongs, or a red-hot poker, and study it; carry it into a dark room, and look at it. You see it for a certain time; after a certain time you cease to see it, but you still perceive radiant heat from it. Well, now, there is radiant heat perceived by the eye and the face and the hands all the time; but it is perceived only by the sense of temperature, when the hot body ceases to be red-hot. There is then, to our senses, an absolute distinction in modes of perception between that which is continuous in the external nature of the thing, namely, radiant heat in its visible and invisible varieties. It operates upon our senses in a way that I cannot ask anatomists to admit to be one and the same in both cases. They cannot now, at all events, say that there is an absolute continuity between the retina of the eye in its perception of radiant heat as light, and the skin of the hand in its perception of radiant heat as heat. We may come to know more; it may yet appear that there is a continuity. Some of Darwin's sublime speculations may become realities to us; and we may come to recognize a cultivable retina all over the body. We have not done that yet, but Darwin's grand idea occurs as suggesting that there may be an absolute continuity between the perception of radiant heat by the retina of the eye and its perception by the tissues and nerves concerned in the mere sense of heat. We must be content in the mean time, however, to make a distinction between the senses of light and heat. And indeed it must be remarked that our sense of heat is not excited by radiant heat only, while it is only and essentially radiant heat that gives to the retina the sense of light. Hold your hand under a red-hot poker in a dark room; you perceive it to be hot solely by its radiant heat, and you see it also by its radiant heat. Now place the hand over it; you feel more of the heat. Now, in fact, you perceive its heat in three ways—by contact with the heated air which has ascended from the poker, and by radiant heat felt by your sense of heat, and by radiant heat seen as light (the iron being still red-hot). But the sense of heat is the same throughout, and is a certain effect experienced by the tissue, whether it be caused by radiant heat or by contact with heated particles of the air.

Lastly, there remains—and I am afraid I have already taxed your patience too long—the sense of force. I have been vehemently attacked for asserting this sixth sense. I need not go into the controversy; I need not explain to you the ground on which I have been attacked; I could not, in fact, because in reading the attack I have not been able to understand it myself. The only tangible ground of attack, perhaps, was that a writer in New York published this theory in 1880. I had quoted Dr. Thomas Reid, without giving a date; his date chances to be 1780 or thereabouts. But physiologists have very strenuously resisted admitting that the sense of roughness is the same as that muscular sense which the metaphysicians who followed Dr. Thomas Reid in the University of Glasgow taught. It was in the University of Glasgow that I learned the muscular sense, and I have not seen it very distinctly stated elsewhere. What is this "muscular sense"? I press upon the desk before me with my right hand, or I walk forward holding out my hand in the dark, and using this means to feel my way, as a blind man does constantly who finds where he is, and guides himself, by the sense of touch. I walk on until I perceive an obstruction by a sense of force in the palm of the hand. How and where do I perceive this sensation? Anatomists will tell you it is felt in the muscles of the arm. Here, then, is a force which I perceive in the muscles of the arm, and the corresponding perceptivity is properly enough called a muscular sense. But now take the tip of your finger and rub a piece of sandstone, or a piece of loaf sugar, or a smooth table. Take a piece of loaf sugar between your finger and thumb, and take a smooth glass between your finger and thumb. You perceive a difference. What is that difference? It is the sense of roughness and smoothness. Physiologists and anatomists have used the word "tactile" sense to designate it. I confess that this does not convey much to my mind. "Tactile" is merely "of or belonging to touch," and in saying we perceive roughness and smoothness by a tactile sense, we are where we were. We are not enlightened by being told that there is a tactile sense as a department of our sense of touch. But I say the thing thought of is a sense of force. We cannot away with it; it is a sense of forces, of directions of forces, and of places of application of forces. If the places of application of the forces are the palms of the two hands, we perceive accordingly, and know that we perceive, in the muscles of the arms, effects of large pressures on the palms of the hands. But if the places of application are a hundred little areas on one finger, we still perceive the effect as force. We distinguish between a uniformly distributed force like the force of a piece of smooth glass and forces distributed over ten or a hundred little areas. And this is the sense of smoothness and roughness. The sense of roughness is therefore a sense of forces, and of places of application of forces, just as the sense of forces in your two hands streched out is the sense of forces in places at a distance of six feet apart. Whether the places be at a distance of six feet or at a distance of one hundredth of an inch, it is the sense of forces, and of places of application of forces, and of directions of forces, that we deal with in the sense of touch other than heat. Now, anatomists and physiologists have a good right to distinguish between the kind of excitement of tissue in the finger and the minute nerves of the skin and sub-skin of the finger, by which you perceive roughness and smoothness, in the one case, and of the muscles by which you perceive places of application very distant, in the other. But whether the forces be so near that anatomists cannot distinguish muscles, cannot point out muscles, resisting forces and balancing them—because, remember, when you take a piece of glass in your fingers every bit of pressure at every ten-thousandth of an inch pressed by the glass against the finger is a balanced force—or whether they be far asunder and obviously balanced by the muscles of the two arms, the thing perceived is the same in kind. Anatomists do not show us muscles balancing the individual forces experienced by the small areas of the finger itself, when we touch a piece of smooth glass, or the individual force in the scores of hundreds of little areas experienced when we touch a piece of rough sugar or rough sandstone; and perhaps it is not by muscles smaller than the muscles of the fingers as a whole that the multitudinousness is dealt with; or perhaps, on the other hand, these nerves and tissues are continuous in their qualities with muscles. I go beyond the range of my subject whenever I speak of muscles and nerves; but externally the sense of touch other than heat is the same in all cases—it is the sense of forces and of places of application of forces and of directions of forces. I hope now I have justified the sixth sense; and that you will excuse me for having taxed your patience so long in not having done it in fewer words.

TWO USEFUL PROBLEMS.

To the Editor of the Scientific American:

I inclose a very neat problem in plane geometry. If any one has ever seen it in any published work, I would like to know it.

Problem: To inscribe a regular polygon of any number of sides in any circle.

Take any circle, with diameter, AB. Let the number of

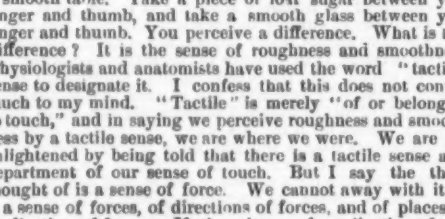


FIG. 1

sides be five. Divide AB into five equal parts. To do this draw any line AE, and upon it lay off five equal parts from a convenient scale (in the figure I made each ¾ of an inch), join E with B and draw lines from the divisions in EA parallel to EB. Their intersections with AB will give the required divisions of that line by the principle of similar triangles.

Now, with A as a center, and a radius AB, describe arcs at D and C. With B as a center, and same radius, describe arcs cutting the former at D, C. From D draw lines through the divisions in AB to the circumference of the circle, and do the same from C. The 10 points so found will be the vertices of a polygon of 10 sides. Joining the alternate points, and we have a polygon of five sides.

The great advantage of this method is that it works equally well for any number of sides from 3 to infinity or to the practical limits of drawing.

The following very useful device is submitted for the benefit of mechanics who wish to cut out metal rollers in the flat material. The rule is, in general, to take a breadth of metal equal to 3·1 times the required diameter. This is defective. The following simple diagram can be readily constructed and used by any mechanic:

Cut out, as perfectly as possible, a circle of metal. Carefully roll this out on a straight line, as BC: then BC will equal the circumference. Construct a right angled triangle, as in the figure, making BA exactly equal to the diameter of the circle. (It is well to make this first diameter an even number, as 10 inches.) Then divide the line BA into equal parts and fractions of a part, and draw the parallel lines 1*a*, 2*b*, etc. If a roller be desired of a certain diameter, go to your scale, look for the diameter in the line BA, and

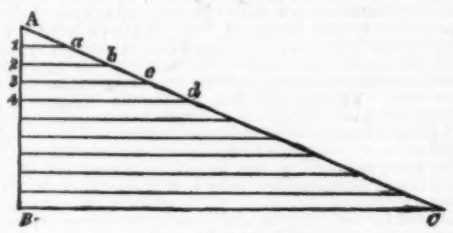


FIG. 2

measure the corresponding line parallel to the base. For example, if you want a roller with diameter of 3 in., take the line 3*c*, it will be the circumference required. Such a scale can readily be constructed of any desired size and upon any material, and hung upon the wall for easy reference.

CAPT. R. KELSO CARTER,
Prof. Higher Math., P. M. A.
Pa. Mil. Academy, Chester, Dec. 5, 1883.

A CATALOGUE containing brief notices of many important scientific papers heretofore published in the SUPPLEMENT, may be had gratis at this office.

THE
Scientific American Supplement.
PUBLISHED WEEKLY.
Terms of Subscription, $5 a Year.

Sent by mail, postage prepaid, to subscribers in any part of the United States or Canada. Six dollars a year, sent, prepaid, to any foreign country.

All the back numbers of THE SUPPLEMENT, from the commencement, January 1, 1876, can be had. Price, 10 cents each.

All the back volumes of THE SUPPLEMENT can likewise be supplied. Two volumes are issued yearly. Price of each volume, $2.50, stitched in paper, or $3.50, bound in stiff covers.

COMBINED RATES.—One copy of SCIENTIFIC AMERICAN and one copy of SCIENTIFIC AMERICAN SUPPLEMENT, one year, postpaid, $7.00.

A liberal discount to booksellers, news agents, and canvassers.

MUNN & CO., Publishers,
361 Broadway, New York, N. Y.

TABLE OF CONTENTS.

PAGE

I. ENGINEERING AND MECHANICS.—Bange's Artillery System.—Siege and fortress guns.—3 engravings..... 6967
Planetary Wheel Trains.—Action of curious combinations of wheels, and ormuln or equations for the same.—With 15 figures.. 6968
Single Spindle Profiling Machine.—With engraving..... 6968
Proposed Suez Ship Railway.—With full page of engravings..... 6970
Auber's Tubular Boiler with Movable Fire-box.—1 engraving..... 6972
Submarine Explorations.—Apparatus used on the Talisman for collecting water at different depths.—Electric light in the sea.—Effect of the pressure of the water at great depths.—Deep sea and surface fishes taken by the Talisman..... 6976

II. TECHNOLOGY.—Notes on Pharmaceutical Apparatus.—Paper read by CHAS. SYMES at a meeting of the Liverpool Chemists' Association.—With 3 engravings..... 6972
Amalgam Retort and Condenser.—With engraving..... 6978
Lawrence's Capillary Coolers.—3 figures..... 6983

III. ELECTRICITY.—The Play of the Needle.—With 1 figure..... 6973
The Great Discoveries in Electricity..... 6973
History of the Electric Telegraph.—How Morse first thought of the telegraph.—Description of Morse's different apparatus.—With five engravings and diagram showing specimen of writing of Morse's first telegraph 6974
The Drawbaugh Telephones.—With full description and ten figures..... 6975
The Source of Atmospheric Electricity.—Hypotheses of different scientists.—By Prof. TAIT..... 6976

IV. ASTRONOMY.—Prof. HEYER on Comets.—A letter to the Editor, 6977

V. PHYSIOLOGY, ETC.—The Six Gateways of Knowledge.—By Prof. Sir WM. THOMSON.—Treating of the senses, manner in which they are affected, etc.—A sixth sense..... 6980

VI. MISCELLANEOUS.—How to Make an Incubator.—Directions.—Brooders.—6 figures..... 6977
Two Useful Geometrical Problems..... 6982

PATENTS.

In connection with the **Scientific American,** Messrs MUNN & Co. are Solicitors of American and Foreign Patents, have had 30 years' experience, and now have the largest establishment in the world. Patents are obtained on the best terms.

A special notice is made in the Scientific American of all Inventions patented through this Agency, with the name and residence of the Patentee. By the immense circulation thus given, public attention is directed to the merits of the new patent, and sales or introduction often easily effected.

Any person who has made a new discovery or invention can ascertain, free of charge, whether a patent can probably be obtained, by writing to MUNN & Co.

We also send free our Hand Book about the Patent Laws, Patents, Caveats, Trade Marks, their costs, and how procured. Address

MUNN & CO., 361 Broadway, New York.
Branch Office, cor. F and 7th Sts., Washington, D. C.

Detroit Public Library